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The 1944 Values of Certain Atomic Constants with Particular Reference to the Electronic Charge*

RAYMOND T. BIRGE
University of California, Berkeley, California

IN 1941 I wrote, on request, a relatively brief paper on the values of the general physical constants.¹ Since then additional calculations have been carried out on the Faraday, the Avogadro number, and the electronic charge. The results of these latter investigations have been reported in a series of papers^{2,3,4} before the American Physical Society, but no detailed account has hitherto appeared in print. The present paper is based primarily on that work, and includes also some quite recent calculations. These calculations are as yet unfinished, but there are certain results and conclusions now ready that should be of some interest.

THE OIL-DROP DETERMINATION OF e

It is more than a quarter of a century since Millikan carried out his justly famous oil-drop determination of the value of the electronic charge, and much has happened since then in this

general field. In particular, it is now possible to calculate the value of e in various indirect ways. In fact, the chief interest in the values of the *atomic* constants results from the numerous theoretical relations in which they appear. Certainly my own chief interest lies in testing just such relations, and the work on other constants has been undertaken mainly because such additional quantities enter as auxiliary constants in the calculation of the atomic constants. It is really difficult for one who has not worked in the field to visualize the maze of interrelations that exists between the general physical constants.

At the present time the most precise, and I believe also the most accurate method of determining e is by means of the ratio of the Faraday to the Avogadro number. The final result, which will presently be discussed in some detail, is $(4.8021 \pm 0.0006) \times 10^{-10}$ abs-esu. Thus the probable error is only 125 parts per million.⁵ This determination may be contrasted with the value obtained from Millikan's own oil-drop work, but with the use of the present best average value for the coefficient of viscosity of air. This resulting value of e is 4.8071 ± 0.0055 , and it thus differs from the indirect value by slightly less than its own probable error. But this probable error is *nine* times as large as that just quoted for the indirect determination, and it is worthy of

* Slightly revised version of an invited paper, delivered at the Pasadena meeting of the American Physical Society, December 16, 1944.

¹ R. T. Birge, "The general physical constants as of August 1941 with details on the velocity of light only," *Reports on progress in physics*, vol. 8 (1942), pp. 90-134. This paper will be denoted G.C. 1941.

² R. T. Birge, *Phys. Rev.* **61**, 204 (1942). The value of the Faraday.

³ R. T. Birge, *Phys. Rev.* **61**, 206 (1942). The value of the electronic charge.

⁴ R. T. Birge, *Phys. Rev.* **62**, 301 (1942). The properties of certain crystals, and the value of the Avogadro number N_0 .

⁵ In the present paper, *all* proportional probable errors, for ease in comparison, will be stated in *parts per million*.

note that the probable error in the determination of the coefficient of viscosity of air contributes slightly more than does the probable error of the oil-drop work itself.

It will doubtless be possible to improve considerably the accuracy of the oil-drop work, perhaps along the lines initiated by Hopper and Laby in 1941, but I feel rather pessimistic regarding any great improvement in the viscosity of air work. Hence it now seems difficult, if not impossible to achieve by oil-drop methods a *resulting* accuracy in the determination of e in any way commensurate with that already obtained by at least one *indirect* method. It is for just this reason that a detailed analysis of the indirect calculations of e becomes of primary importance. I may interject, at this point, that Hopper and Laby claim a far greater accuracy for their oil-drop work than that attained by Millikan, but a critical review of their work fails to substantiate this claim, as will be shown presently.

I have just remarked that the indirect value of e will be discussed in some detail. That is really a gross exaggeration, for an actually detailed account of my calculations, covering the numerous required auxiliary constants and the reasons for the finally adopted value of each, would fill an inordinate number of printed pages. One may wonder why I go so far back of the scenes, endeavoring in all cases, as far as possible, to start with the investigator's actual observations. Originally this was done because it was noted that, in the reduction of the observations to obtain a final published result, one investigator had used one set of values of the auxiliary constants, and another investigator working on the same subject had used a quite different set. Obviously the two published results are not properly comparable until they have been recomputed in terms of a common set of auxiliary constants, and moreover, often new and more trustworthy values of certain of the auxiliary constants had since become available. The coefficient of viscosity of air, required in connection with the oil-drop determination of e , is a notable example of such an auxiliary constant.

But although my original object was thus merely to bring about *consistency* in the field, I have, as a result of such detailed recalculation, occasionally come across serious errors in the published reduction of the data—sometimes errors of theory, and in other cases straight arithmetic mistakes. Naturally I am both sur-

prised and shocked at such discoveries, and I am often placed in a most embarrassing position. But the only point I am making now is that it seems necessary, in all serious work of a critical nature, to take nothing for granted. In fact, the great complaint of everyone who attempts to do such work is that the average investigator—often, to be sure, under pressure from the editor—fails to include sufficient detailed observational material in his published paper to make even possible a valid recalculation of his results. I note, in this connection, that Millikan's oil-drop work represents one of the few instances where full experimental data have been published, and his data were worked over in great detail by myself, in 1929.

I turn now to the oil-drop work of Hopper and Laby⁶ for which, as already noted, a far greater accuracy is claimed than in the case of Millikan's work. These investigators used a *horizontal* electric field, and introduced automatic time recording, photographic observation of drop position, and other improvements over Millikan's procedure that should make possible a very precise evaluation of e . They used 16 drops for their final result, and since the $1/pa$ -values were much smaller than those used by Millikan, the necessary extrapolation to obtain the intercept of the e^2 -curve was much shorter. From their *published* values of e^2 and $1/pa$ for each drop, but with my own adopted value of the coefficient of viscosity of air, namely $(1832.5 \pm 1.0) \times 10^{-7}$ cgs units, at 23°C , I find $e = (4.8119 \pm 0.0013) \times 10^{-10}$ esu. The probable error is merely that of the least squares' solution of their data. The corresponding probable error for the least squares' solution of Millikan's data is ± 0.0038 , practically *three* times as large.⁷

In studying the Hopper and Laby paper, I noted immediately a number of errors. Many of these were obviously typographical errors, but others appeared to be real mistakes. Thus in their Table 7, drops No. 2 and No. 9 (both Apiezon oil) are listed as having the same temperature and hence also requiring the same value for the coefficient of viscosity of air, but quite different values are listed for the corresponding densities. As a result of this situation, I was led to make a complete recalculation of all of their work, so far as the published data permitted. The results of

⁶ V. D. Hopper and T. H. Laby, Proc. Roy. Soc. A178, 243 (1941).

⁷ Hopper and Laby actually adopt a coefficient of viscosity value of 1830 and thus get $e = 4.8020$, in *perfect* agreement with the present adopted *indirect* value of e .

this recalculation cast an entirely new light on the accuracy of their experimental work, and because of the significance of this, in connection with the best oil-drop value of the electronic charge, it seems necessary to report some of the details.

Hopper and Laby observe photographically the position of a drop, at equal time intervals, when first falling under gravity alone, and then with the added influence of a horizontal electric field. They do not, however, *assume* the electric field to be horizontal. Instead, from the measured component of drift in the vertical direction, with and without the electric field (s_g' and s_g respectively) and from the measured *resultant* drift with the field on (s_r), they *calculate* the deviation of the electric field from the horizontal.

Now it would seem easily possible to place the condenser plates vertical, within a fraction of a degree, and such a small deviation could then be ignored in the calculation of e . Furthermore, if the apparatus were not disturbed between runs, the value of the deviation δ , whatever it was, should remain *constant*.⁸ Actually, the value of δ , as *calculated* from their data, turns out to be *different* for each drop, with the surprisingly large extreme values of -3.3° and $+2.1^\circ$, with equal numbers of plus and minus values, and with an average absolute value of 0.94° . These figures do *not* include drop No. 4, for which the full detailed observational data are alone published. In the case of this drop their printed value of s_x (the calculated drift in the direction of the electric field) is seriously in error, and corresponds⁹ to a value of $\delta = 4^\circ 40'$.

Their published average values of s_g , s_g' and s_r for drop No. 4 are, I find, just the *arithmetic averages* of the various sets of first differences (of the positions of the drop). Such a method of averaging first differences, as is well known, automatically cancels out all but the first and last measurements of position. For drop No. 4, for which alone such individual readings of position are given, I find that when the true least

⁸ Hopper and Laby virtually admit this, since they state that they measured all *three* drifts (s_g , s_g' and s_r) for each drop (instead of only two of the drifts) merely in order to check the value of δ obtained from the first measured drop. They do not, however, publish the resulting value of δ for any drop, and hence make no comment on the surprising distribution of values of δ .

⁹ The true value of δ for this drop, from their own data, is $28'$, and their error corresponds just to a ten-fold mistake in the value of $\tan \delta$. This mistake is, I believe, in some way connected with the fact that in the tables, all values of drifts are given in millimeters, but in the sample calculation for drop No. 4 they are given in centimeters. In the results about to be presented, this error in δ and the consequent error in s_x for drop No. 4, have been corrected.

squares' averages of the three sets of first differences are used, the value of e is *increased*, so far as this drop alone is concerned, by 1710 parts per million, which is over *six* times the claimed probable error of their final result! On the other hand, the correction for their mistake in the value of δ *decreases* e by 3280 parts per million.

In the case of all other drops one can only accept their published average values for the three measured drifts since the individual first differences are not given. Then the resulting apparent value of e is given by an expression containing 13 factors. Each of these factors was checked, so far as it was possible, and the complete necessary calculation for e in the case of each of the 16 drops was then made.

For drop No. 4, with their *own* values of each factor, my calculated e is 3460 parts per million *higher* than theirs. The final result of all three mistakes in drop No. 4 is then to make their published value of e , for this drop, too low by 1890 parts per million. Furthermore, I get a resulting value of e in substantial agreement with theirs in the case of only *four* (Nos. 2, 7, 11, 12) of the 16 drops. What is even more important, my own calculated values for the 16 drops deviate from the resulting least squares' straight line by an average amount that is just *three times* the average deviation for the least squares' solution of their own calculated points (a solution that I have checked and found entirely correct). In fact, *half* of my calculated residuals are *larger* than any one of their *published* residuals!

The resulting average residual per drop, for their work, is still only 40 percent of that in the case of Millikan's 25 drops. Hopper and Laby also achieve increased accuracy by using large drops, so that their abscissa values, as noted, are all smaller than those of Millikan and a shorter extrapolation is accordingly required in order to obtain the desired intercept at $1/pa = 0$. But the greater accuracy thus achieved by Hopper and Laby is nearly cancelled by the fact that their range of abscissa values is only one-fifteenth of that in the case of Millikan's work.

My final calculated value of e , from the recomputed Hopper and Laby data, is

$$e = 4.8137 \pm 0.0030 \text{ (622 parts per million)}$$

as compared to their published

$$e = 4.8119 \pm 0.0013 \text{ (270 parts per million),}$$

where both results have been reduced to my present adopted value of the coefficient of vis-

cosity of air, but the stated probable errors do not include any allowance for the probable error of this factor.

Thus I have *raised* the Hopper and Laby value of e by 374 parts per million, which is, to be sure, only 60 percent of their true probable error of 622 parts per million. But we have already seen that there is an error of 1710 parts per million (nearly *three* times the apparent final probable error), for drop No. 4, just because of the use of *arithmetic* averages, in place of least squares' averages, for measured drifts, and because of lack of published data, there is no way of making the corresponding calculation for the other 15 drops. It is doubtful if such a calculation, when made, will produce a really significant change in the final *probable error* in e , but it may easily change the resulting *value* of e by *at least* another 374 parts per million. Just because of this remaining uncertainty, the *same* weight has been assigned to their result, as recalculated, as to Millikan's result, which, in terms of my adopted value of the coefficient of viscosity of air (but again ignoring the probable error in this value), is

$$e = 4.8071 \pm 0.0038 \text{ (790 parts per million).}$$

His result is thus 1370 parts per million *below* that of Hopper and Laby.

These two experimental results of oil-drop work on e , together with two additional results, are collected in Table I. In all four cases the listed probable errors are merely those of the least squares' solution of the e^3 versus $1/pa$ linear plot, with no allowance for systematic errors of any kind. Hence there is no logical basis for assigning weights in strict accordance with the stated probable errors. A detailed study of the Ishida results has not yet been made, but I greatly doubt if they have the accuracy claimed. Certainly the very high resulting value of e , as compared to the

TABLE I. Values of e from oil-drop work (calculated with $\eta_{23} = 1832.5 \times 10^{-7}$ cgs units).

Author	Result	Assigned weight
Millikan	4.8071 ± 0.0038	2
Hopper and Laby	4.8137 ± 0.0030	2
Bäcklin and Flemberg ¹⁰	4.7909 ± 0.0114	1
Ishida, <i>et al.</i> ¹¹	4.8453 ± 0.0030	1
Weighted average	4.8130 ± 0.0064	

¹⁰ E. Bäcklin and H. Flemberg, *Nature* **137**, 655 (1936).

¹¹ Y. Ishida, I. Fukushima and T. Suetsugu, *Sci. Papers, Inst. Phys. and Chem. Res. Tokyo* **32**, 57 (1937).

values given by the other three investigations, lends at least some grounds for suspicion. What has finally been done is to give the arbitrary weights listed in Table I. One then gets as the final weighted average for *all* oil-drop work,

$$e = 4.8130 \pm 0.0064 \text{ (1330 parts per million),}$$

exclusive of any error in the coefficient of viscosity of air. The probable error is necessarily based on *external* consistency, since arbitrary weights have been assigned. But the probable error of the weighted average is *twice* as large as the adopted probable error, in the case of three out of the four individual results, showing directly that serious systematic errors are present somewhere. This lack of consistency would, however, almost vanish if the Ishida result were discarded, the new weighted average being then 4.8065 ± 0.0040 , almost identical in both value and probable error with Millikan's own result.

Time prevents a detailed discussion of experimental work on the coefficient of viscosity of air. I present here merely a table of the more recent experimental results. None of the older results seems worthy of inclusion. Each of the adopted experimental values listed in Table II has been reduced to 23°C by means of the Sutherland equation for the variation of coefficient of viscosity with temperature.¹²

Of the six results listed in Table II, that by Bearden can seemingly claim the highest accuracy. Hopper and Laby state that Bearden made an incorrect calculation of his probable error, thereby greatly underestimating it. But it is Hopper and Laby who are wrong. Bearden's calculations of probable error are technically correct, although they include no allowance for systematic error and hence his assumed probable error should certainly be increased. The arbitrary weight now assigned to each result is shown in Table II.

One thus obtains for the weighted average value of the viscosity of air,

$$\eta_{23} = (1832.45 \pm 0.69) \times 10^{-7} \text{ cgs units.}$$

Slightly increasing the probable error, I *adopt*

$$\eta_{23} = 1832.5 \pm 1.0, \text{ as already mentioned.}$$

¹² Hopper and Laby (ref. 6) call attention to the inaccuracy of the ordinarily assumed linear relation with temperature, and also make a recalculation, where necessary, in terms of the Sutherland equation. Their Table 3 includes several of the older determinations, and lists their own attempted correction of Kellström's 1937 value, whereas Table II gives Kellström's own 1941 correction. Otherwise the recalculated results, as given in the table of Hopper and Laby, are in fair agreement with my own.

All the quoted values of e , from oil-drop work, are in terms of this latter figure for the coefficient of viscosity of air at 23°C. When the adopted probable error of 547 parts per million for the viscosity—or 820 parts per million for its $\frac{2}{3}$ power (since e varies with the $\frac{2}{3}$ power of the viscosity)—is combined with the adopted probable error in e of 1330 parts per million, for the oil-drop work itself, we get a final probable error of 1560 parts per million, that is,

$$e = (4.8130 \pm 0.0075) \times 10^{-10} \text{ esu (final oil-drop average).}$$

This probable error is *over 12* times as great as that quoted earlier for the *indirect* evaluation of e from the ratio F/N_0 , and to such an indirect evaluation we now turn.

THE AVOGADRO NUMBER N_0

As contrasted with the continuing difficulties in getting a reliable value of e from oil-drop work, the recent experimental results from which one can calculate a value of the Avogadro number N_0 have shown a gratifying increase in precision, and the individual determinations of N_0 from various types of crystals show an equally gratifying consistency. In fact, I believe that N_0 is now one of the best established of the general physical constants. My present weighted average value, based on results from *five* different varieties of crystal, is

$$N_0 = (6.02338 \pm 0.00043) \times 10^{23} \text{ mole}^{-1},$$

on the chemical scale of atomic weights. The

TABLE II. Coefficient of viscosity of air (at 23°C, in 10^{-7} cgs units).

Method	Author	Published result	Corrected result (Sutherland Eq.)	Author's claimed uncertainty	Assigned weight
Rotating cylinder	Bearden ¹³	1834.12	1833.79	0.06	5
	Houston ¹⁴	1829.2	1829.13	4.5	3
	Kellström ¹⁵	1835.2	1835.00	3.0	2
Capillary tube	Bond ¹⁶	1834.7	1834.34	0.8	1
	Rigden ¹⁷	1830.34	1829.96	0.7	2
	Banerjee and Plattanaik ¹⁸	1833.3	1833.75	2.2	1
Weighted average		1832.45 \pm 0.69			

probable error is thus only 71 parts per million, as contrasted with the G. C. 1941 value (based on the properties of calcite alone) of

$$N_0 = (6.0228_3 \pm 0.0011) \times 10^{23} \text{ mole}^{-1},$$

for which the probable error is 2.6 times as large, although the *change* of value is well within the limits of that probable error.

As in the case of the oil-drop value of e , one could write at great length concerning the evaluation of N_0 . Actually I shall allude but briefly to the various factors that enter into its determination, and to certain of the sources of error in each factor.

When one stops to consider the matter, it does seem rather remarkable that one can obtain any *quantitative* measure of the actual number of *molecules* in a specified mass of a given material. But Virgo,¹⁹ in an interesting article, discusses at least 20 different methods, and quotes 72 different determinations of N_0 . The present most accurate method seems, in a way, strangely indirect. We measure the atomic weights of certain elements, the density of certain crystals composed of these elements, the wave-lengths (by means of a ruled grating) of certain x-ray lines, and finally, the Bragg angle at which these same x-ray lines are reflected from the cleavage surfaces or other lattice planes of the crystals in question.

The theory is, however, very simple. If one edge of the "unit cell" in a cubic crystal is denoted by d , and the density of the crystal by ρ , then $d^3\rho$ is the mass in a unit cell. If there are f molecules in a unit cell,²⁰ then $d^3\rho/f$ is the mass per molecule, and if M is the molecular weight, the *number of molecules in a gram mole*—which defines the Avogadro number N_0 —is given by the ratio of these last two quantities. For crystals, in general, the volume of a unit cell is given by ϕd^3 , where ϕ is a geometric factor. The general formula for the Avogadro number is thus

$$N_0 = Mf/\rho\phi d^3.$$

The five crystals from which we can now obtain precision values of N_0 are calcite, rock salt, diamond, lithium fluoride, and potassium chloride. Of these, all but calcite belong to the

¹³ J. A. Bearden, Phys. Rev. **56**, 1023 (1939).

¹⁴ W. V. Houston, Phys. Rev. **52**, 751 (1937).

¹⁵ G. Kellström, Phil. Mag. **23**, 313 (1937) and **31**, 466 (1941).

¹⁶ W. N. Bond, Proc. Phys. Soc. **49**, 205 (1937).

¹⁷ P. J. Rigden, Phil. Mag. **25**, 961 (1938).

¹⁸ G. B. Banerjee and B. Plattanaik, Zeits. f. Physik **110**, 676 (1938).

¹⁹ S. E. Virgo, Sci. Progress **27**, 634 (1933).

²⁰ It is customary to use the word "molecule" in this connection, and purely for convenience this custom is continued here. But it is well known that no molecules, in the ordinary sense of the word, exist in an ionic crystal lattice. What is actually meant is an "ion-pair" or an "elementary ion-group," with a definite "combining weight," rather than a molecular weight.

cubic system, for which ϕ is unity. For NaCl, KCl, and LiF, the number of molecules f in a unit cell is four. But it is customary, in calculating N_0 , to use for d a quantity equal to one half the edge of the unit cell, since x-rays give a first-order spectrum by reflection from successive atomic layers at this smaller separation (approximately 2.8Å for rock salt), which is called the crystal grating constant. Hence f must be taken as $\frac{1}{2}$ instead of 4. In the case of diamond, the side of the unit cell is roughly 3.56Å, and $f=8$. But x-ray reflection can be obtained at one fourth of this separation, and with this *new* value of d , $f=\frac{1}{8}$. The crystallographic description of diamond has changed during the past 20 years, but I will not go into the matter.

In the case of calcite, the unit cell contains four molecules of calcium carbonate, and the corresponding separation of the planes used in x-ray reflection is roughly 6.06Å. But just as in the case of rock salt, one can obtain first-order x-ray reflection at half this distance, and for this smaller amount $f=\frac{1}{2}$. Furthermore, the unit cell of calcite is a rhombohedron, of volume ϕd^3 , where ϕ is a known function of β ($\sim 101^\circ 54'$), the obtuse angle between the edges of the crystal; that is,

$$\phi = \frac{(1 + \cos \beta)^2}{\sin \beta (1 + 2 \cos \beta)}.$$

The angle actually measured is α ($\sim 105^\circ 3.5'$), the obtuse angle between the faces, and²¹

$$\cos \frac{1}{2}\beta = (2 \sin \frac{1}{2}\alpha)^{-1}.$$

The greatly increased accuracy now possible in the determination of N_0 has resulted, primarily, from the "free submerged float" method of obtaining densities of crystals, which has been developed to a high degree of perfection just during the past few years. Using this method, Bearden²² determined the density of diamond, in 1938. Then C. A. Hutchison and Johnston,²³ in 1940, determined the density of chemically pure LiF crystals, and in 1942 Johnston and D. A. Hutchison²⁴ determined the density of chemically pure NaCl crystals. Finally, just a few months ago, D. A. Hutchison²⁵ published a similar result for pure KCl crystals. It is this recent result that

has enabled me to add KCl to the four crystals used in my report⁴ on N_0 to the American Physical Society, two years ago. The actual accuracy obtained for the density, as well as for the other factors entering into the determination of N_0 , is given, for each of the five crystals, in Table IV ahead.

The crystal grating space d is obtained from the Bragg reflection law for x-rays, by means of the absolute wave-lengths of x-rays determined by ruled gratings, and observed values of the glancing angle at the surface of each crystal under consideration, suitably corrected for refraction. For calcite Bearden's fourth-order reflection data²¹ are used, and for rock salt, diamond and KCl the results of Tu.²⁶ For LiF I use the results of Straumanis, Ievins and Karlsons,²⁷ as obtained by the so-called "powder" method. This method, which must be employed when large crystals with perfect cleavage surfaces are not obtainable, certainly yields less trustworthy results than does the Bragg reflection method, in spite of enormous improvements by Straumanis and his co-workers. In fact, the probable error in the value of d^3 for LiF, is the *largest* of any factor in the equation for N_0 , for any one of the five crystals (see Table IV).

In calculating d , we first get its value on the Siegbahn system of x-ray wave-lengths. We then convert to the cgs system by means of the ratio λ_0/λ_s , that is, the ratio of grating (true) wave-lengths of x-rays to Siegbahn wave-lengths. The *probable error* in this ratio is, however, not applied to the individual evaluations of N_0 , since it affects all results alike. Instead, it is finally compounded with the probable error resulting from all *other* factors, in the *weighted average* value of N_0 , in order to get the final adopted probable error of the weighted average.

Now it is most noteworthy that this final weighted average value of N_0 has a probable error, aside from that in the λ_0/λ_s ratio, of only 38 parts per million, whereas the probable error in the ratio itself is 20 parts per million. Since the cube of the ratio enters into N_0 , the final contribution to the probable error of N_0 is 60 parts per million—nearly *twice* that resulting from all other factors! From the usual square root of the sum of squares [here $(38^2 + 60^2)^{1/2}$], we then get 71 parts per million as the final adopted probable error in the Avogadro number, as already stated.

²¹ J. A. Bearden, Phys. Rev. **38**, 2089 (1931), writes the exponent as $-\frac{1}{2}$, but actually uses the correct formula.

²² J. A. Bearden, Phys. Rev. **54**, 698 (1938).

²³ C. A. Hutchison and H. L. Johnston, J. Am. Chem. Soc. **62**, 3165 (1940).

²⁴ H. L. Johnston and D. A. Hutchison, Phys. Rev. **62**, 32 (1942).

²⁵ D. A. Hutchison, Phys. Rev. **66**, 144 (1944).

²⁶ Y. Tu, Phys. Rev. **40**, 662 (1932).

²⁷ M. Straumanis, A. Ievins and K. Karlsons, Zeits. f. physik. Chemie **B42**, 143 (1939).

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TABLE III. Comparison of grating and Siegbahn wavelengths of x-rays.

Author	X-ray line	(Probable error based on accidental errors only) ($\lambda_0/\lambda_s - 1$) $\times 10^4$	Adopted prob. error
1. Bearden ^a	Cu $K\alpha_{1,2}$	1980 \pm 31	\pm 52
2. Bearden ^a	Cu $K\beta_{1,2}$	2079 \pm 28	\pm 50
3. Bearden ^a	Cr $K\alpha_{1,2}$	2036 \pm 21	\pm 48
4. Bearden ^a	Cr $K\beta_{1,2}$	2017 \pm 28	\pm 48
5. Soderman ^b	Al $K\alpha_{1,2}$	2070 \pm 37	\pm 84
6. Bäcklin ^c	Al $K\alpha_{1,2}$	2000 \pm 14	\pm 60
7. Bearden ^d	Cu $K\alpha_1$	2087 \pm 55	\pm 130
8. Tyrén ^e	Al $K\alpha_{1,2}$	2024 \pm 5	\pm 48
Adopted average		2030 \pm 20	

^a J. A. Bearden, Phys. Rev. **37**, 1210 (1931).^b M. Soderman, Nature **135**, 67 (1935); Diss. Uppsala (1934).^c E. Bäcklin, Zeits. f. Physik **93**, 450 (1935).^d See reference 30.^e See reference 29.

Because of the great importance of the value of the λ_0/λ_s ratio, a detailed recalculation has been made of all of the original observational material. Bearden,²⁸ in 1941, gave a careful discussion of such material, but failed to include Tyrén's beautiful 1938 work.²⁹ As a result of my own calculations, I have adopted a probable error of 20 parts per million, as just stated, in contrast to the probable error of 60 parts, used in G. C. 1941. The latter value is a purely arbitrary figure, obtained by multiplying by a factor of four the probable error of 15 parts per million calculated by Bearden³⁰ in 1935 on the basis of *external consistency only*.

But even the new adopted value of only 20 parts per million is now a conservative estimate. It is based on *internal consistency*, with the same liberal allowance for systematic error in the various individual results as suggested by Bearden²⁸ in 1941. If one uses *external consistency*, one finds that the final probable error is less than 10 parts per million (in place of Bearden's 15 parts). This remarkable situation is presented in detail in Table III.

My present adopted value of λ_0/λ_s is the same as that of Bearden, namely 1.002030. The deviation from unity is thus 2030 ± 20 , in parts per million. Now note in Table III the corresponding weighted average values obtained by various investigators, for various x-ray lines, with the use of quite an assortment of ruled gratings. The table lists the various probable errors based on

accidental errors only, as well as the final *adopted* probable errors, which include a liberal allowance for systematic error, as already noted. The extreme variation is 107 parts per million. The average deviation from the weighted average is only 31 parts per million, as compared to an average probable error (based on accidental errors only) of 27 parts per million. In other words, the individual results are quite consistent, even with *no* allowance for systematic error! Under these conditions I think it must be admitted that 20 parts per million is a very conservative estimate for the probable error of the final weighted average of these eight results.

In considering the density ρ and the crystal grating space d , there is another possible source of error that has been overlooked by most observers, namely, the uncertainty in the coefficient of thermal expansion! It is customary to reduce to 20°C all quantities used in the evaluation of N_0 , and I have followed the custom. It is, however, quite possible that the choice of another temperature might reduce the final uncertainty in N_0 due to this source of error.

As an illustration of the effect on N_0 of uncertainty in thermal coefficients, consider the density of KCl. D. A. Hutchison²⁵ measures the density at 28°C, reduces it to 25°C, and assigns a *total* probable error of only 10 parts per million to his reduced 25°C result. But there appears to be an uncertainty, in such a reduction, of fully four parts per million, per degree, and hence of 32 parts per million for the reduction from 28°C to 20°C, necessary in the evaluation of N_0 . In the case of KCl, as in the case of other crystals, I have multiplied by a factor of two or three the probable error *assigned* to the density by the original investigator, because, in general, too little allowance appears to have been made for systematic errors. I have then also taken into consideration the probable error of the thermal coefficient.

In the case of calcite the most precise determination of the density, by Bearden,²¹ was deliberately made at 20°C, in order that the coefficient of volume expansion might not enter into the calculation of N_0 . Calcite has two primary coefficients of *linear* expansion, one α_p for expansion parallel to the optic axis, and the other α_s for expansion perpendicular to it. All desired special coefficients may be expressed as functions of α_p and α_s . For expansion along the direction of the crystal grating space d , which makes an angle of *approximately* 45° with the optic axis, Cooksey

²⁸ J. A. Bearden, J. App. Phys. **12**, 395 (1941).²⁹ F. Tyrén, Zeits. f. Physik **109**, 722 (1938).³⁰ J. A. Bearden, Phys. Rev. **48**, 385 (1935).

and Cooksey²¹ made the first precise calculation in terms of α_p and α_s . They assume that their numerical result of $1.023 \times 10^{-5} \text{ deg}^{-1}$ at 18°C (based on Benoit's 1888 values of α_p and α_s) is reliable to less than 1 percent. But Austin and co-workers²² have quite recently shown that different specimens of calcite give definitely different values of α_p and α_s , and the resulting coefficient for d , at 18°C , may in fact be anything from 0.8 to $1.20 \times 10^{-5} \text{ deg}^{-1}$. The best crystals give a value in the vicinity of $1.04 \times 10^{-5} \text{ deg}^{-1}$, and this figure is accordingly adopted, but with a probable error of 10 percent. Bearden made no actual determination of thermal expansion, for his own crystals, but he too used the value 1.04 in reducing his glancing angles to the standard 18°C temperature of the Siegbahn system.

The variation with temperature of the obtuse angle β between the edges of the calcite crystal is given by a rather complicated function of α_p and α_s , apparently first derived by Beets.²³ But he prints as $\frac{2}{3}$ a certain factor that should be $\frac{4}{3}$, and the error is repeated by Bearden.²¹ Both investigators use the same *numerical* value of $d\beta/dt$, and fortunately one that corresponds to the *correct* formula, although without any consideration of its probable error. I mention these details merely as a small example of the difficulties that beset the really critical investigator. He must seemingly not only recalculate all results, but he must also rederive unusual formulas, if he is to feel fully assured of the correctness of the work.

Turning now to the last remaining factor, the molecular weight of the crystal, I first remark that the mass spectrograph values have been used for atomic masses—properly combined, where necessary, with the abundance ratio of isotopes, and reduced to the chemical scale—in the case of carbon, calcium, sodium, lithium and fluorine, and the *international chemical* atomic weights have been used *only* for chlorine and potassium, for which the mass spectrograph values have less precision. Now it is most interesting to note that *if* one uses the international atomic weights for *all* atoms, the final weighted average value of N_0 is raised by only 18 parts per million, but the *average deviation* from the

weighted average, for the five crystals, is increased from 42 to 110 parts per million! Hence the mass spectrograph results for light atoms, combined with the measured abundance ratios of isotopes, certainly seem to form a more *consistent* system of atomic weights than do the values determined chemically.

The only other point to be mentioned in regard to molecular weight concerns the question of the purity of the various crystals. In previous work on the value of the Avogadro number, everyone, including the writer, has used for the molecular weight of calcite that for pure CaCO_3 . It is, however, well known to crystallographers that calcite always contains impurities,²⁴ and Bearden²¹ made a careful chemical analysis and density determination of each of the six specimens that he used in his x-ray reflection experiments.

Now all available evidence indicates that for the very small percentage of impurity found in the most perfect crystals of calcite, an atom of impurity merely replaces the original atom in the space lattice, without any resulting change in the general lattice spacing, at least to first order effects. With this assumption it follows that the observed *density* should be directly proportional to the "*effective*" molecular weight of the crystal, as calculated on the basis of the chemical analysis.

In the case of Bearden's six crystals, I find that the observed impurities (mainly iron and manganese, assumed to replace calcium in the lattice) *increase* the molecular weight by an average of 31 parts per million, and this change increases the resulting value of N_0 in the same proportion. For the six specimens, the impurity effect varies only from 28 to 38 parts per million. To be very conservative, I have assigned to this correction a probable error equal to its own entire magnitude, but it may be noted that the correction for impurity is responsible for the major portion of the increase of the 1941 value of $N_0 = 6.02283$ (based on calcite alone) to the present calcite value of 6.02314—an increase that brings calcite into definitely better agreement with the other five crystals. (See Table IV.)

In the case of the densities of NaCl, KCl and LiF, crystals of the highest possible chemical purity were artificially prepared for the work. Hence we are justified in these cases in using the corresponding molecular weight, without modifi-

²¹ C. D. Cooksey and D. Cooksey, Phys. Rev. **36**, 85 (1930).

²² J. B. Austin, H. Sañi, J. Weigle and R. H. H. Pierce, Jr., Phys. Rev. **57**, 931 (1940).

²³ H. N. Beets, Phys. Rev. **25**, 621 (1925).

²⁴ I am indebted to Dr. A. Pabst for pointing out the significance of this fact in the evaluation of N_0 .

TABLE IV. Probable errors, in parts per million, for factors of N_0 .

Crystal	M	ρ_{20}	d_{20}^2	$\phi(\beta)$	Resulting error in N_0	Resulting value of N_0 (10^{23} mole $^{-1}$)
calcite	50	37	16.5	13	65	6.02314 ± 0.00039
NaCl	51	37	60	—	87	6.02401 ± 0.00052
diamond	20	34	89	—	97	6.02343 ± 0.00059
LiF	32	45	120	—	132	6.02319 ± 0.00080
KCl	40	37	68	—	86	6.02324 ± 0.00052
Adopted weighted average						6.02338 ± 0.00023

cation. It should be remembered, however, that in the case of NaCl, the x-ray reflection work was done by Tu²⁶ with rock salt crystals, and owing to the potassium impurity doubtless present, the lattice constant of rock salt may be slightly different from that of pure NaCl crystal. In the case of diamond there should be no question as to its purity.

With this all too brief discussion of the multitude of problems that have arisen in connection with the evaluation of N_0 , I now give in summarized form (Table IV) the adopted probable errors, in parts per million, for the several factors, and for each crystal. It seems to me both remarkable and fortunate that all of these probable errors are so closely of the same magnitude.

As already stated, the weighted average is 6.02338 ± 0.00023 , where the adopted probable error of 38 parts per million is based on internal consistency. But the error is only 18 parts per million by external consistency, thus suggesting that we have made too liberal an allowance for systematic error, when assigning probable errors to the several factors. Then, with the inclusion of the probable error of 60 parts per million, for the $(\lambda_g/\lambda_e)^3$ factor, as already discussed, one obtains for the final result,

$$N_0 = (6.02338 \pm 0.00043) \times 10^{23} \text{ mole}^{-1} \text{ (71 parts per million).}$$

It should be emphasized that if one uses only external consistency in the case of the final averages of N_0 and λ_g/λ_e , the probable error would be less than half as large as that actually adopted.

Possibly the most important conclusion to be drawn from the remarkable consistency of the results from the five different crystals is that such crystals must be geometrically perfect, to better than one part in 10,000. If there were a mosaic structure of the type originally postulated by Zwicky,²⁵ it would indeed be strange if such a

structure produced exactly the same proportional error in the evaluation of N_0 , for quite different types of crystal structure.

THE FARADAY

In conclusion, I will discuss briefly the value of the Faraday F , first as measured directly by voltameter work, and second as measured indirectly. The voltameter value deserves discussion in some detail, but this would require so much space that I shall merely refer to the rather brief discussion given on pages 112–115 of G. C. 1941.

The gist of that discussion is to the effect that, although the values of F obtained from the silver and from the iodine voltameter differ by some 4.5 times the apparent probable error of the difference, a careful consideration of various possible sources of systematic error in both voltameters, and a correction of each result for certain known types of systematic error, bring the two results closer together and also increase the probable errors of both. The final discrepancy is thus reduced to only $1\frac{1}{2}$ times its probable error, which is not at all unreasonable.

Under such circumstances it becomes entirely legitimate to adopt a weighted average result for the two voltameters. The calculated probable error of the average is only 3 coul, but just because of the possibility of systematic error, I have increased the probable error to 10 coul. The final adopted value, as given in G. C. 1941, is then

$$F = 96487.7 \pm 10 \text{ abs-coul (chem. scale) (104 parts per million).}$$

This result, when combined with the latest value of the Avogadro number, with its probable error of 71 parts in a million, leads to the present most reliable value of e , namely,

$$e = (4.8021 \pm 0.0006) \times 10^{-10} \text{ esu (125 parts per million),}$$

as quoted at the very start of this paper.

One might, at this point, terminate the discussion of the value of e . But I discovered, some years ago, that several entirely reputable scientists have no confidence whatsoever in the voltameter value of the Faraday. They are even willing to assume it to be in error by two or three hundred coulombs, if necessary, in order to bring about greater consistency among the general constants—especially the radiation constants.

It therefore seemed to me highly desirable to calculate the best indirect value of the Faraday, for even if the probable error of such an indirect

²⁵ F. Zwicky, Proc. Nat. Acad. Sci. 16, 211 (1930).

value turned out to be larger than 10 coul, one might get at least some idea of the allowable uncertainty in the voltameter value. Accordingly, an entirely general method for obtaining such a result has been devised, and my first report on the subject² was made three years ago, as already mentioned. Quite recently a few further calculations have been carried out, but the entire investigation should be repeated, in order to include new data—in particular, the latest precision evaluation²⁶ of h/e .

The indirect evaluation of the Faraday involves *every known function* of the *three* atomic constants, that is, the electronic charge e , the electronic mass m , and the Planck constant h . For just this reason the subject requires many pages even for the most superficial treatment. Here I note merely certain essential facts and results.

In the first place there are at least *nine* known functions of e , m and h —or, more conveniently, of e , e/m and h/e —including, of course, the direct measurements of these three constants—for which we now have precise experimental evaluations. There are also a number of different methods of measuring quantities such as e/m and h/e . If one assumes the correctness of the Bohr formula for the Rydberg constant, all such experimental material may be plotted on the so-called Birge-Bond diagram (I use the name with apologies), and should, if self-consistent, lie on some straight line. The deviations of the plotted points from the best straight line that can be drawn through them show at a glance the character and also the extent of the inconsistencies.

An entirely *general* method for obtaining an indirect value of the Faraday is now the following. We first select *every* $f(e, m, h)$ that does *not* involve F . Such material gives *six* points on the diagram, with different abscissa values—each point representing the weighted average of more or less extensive observational material. We then calculate, by least squares, the best straight line for these six points. The solution then gives us values of e , m and h that are completely independent of the value of the Faraday.

I interject here that this resulting value of e , which includes, of course, the direct oil-drop evaluation as well as extensive additional material, but which is *entirely independent* of both the Faraday and the Avogadro number, is

$$e = 4.803_0 \pm 0.002_1 \text{ (437 parts per million).}$$

²⁶ W. K. H. Panofsky, A. E. S. Green and J. W. M. DuMond, Phys. Rev. **62**, 214 (1942).

This result is fairly consistent with the oil-drop value, and entirely consistent with the ratio F/N_0 value. Its probable error is, strangely enough, *precisely* the geometric mean of the adopted probable errors of these other two evaluations of e !

Returning to the indirect evaluation of the Faraday, we now substitute our newly calculated values of e , m and h in *each* $f(e, m, h)$ that *does* involve F . Each such substitution thus yields a value of F . There are *six* different functions of this character, and in the case of one of these functions—that involving the spectroscopic fine-structure evaluation of e/m —there are at least *six* different precision results, *including* that by Hsueh.²⁷ From all such indirect evaluations of F one finally gets a weighted average value and a calculated probable error.

A very special, but very simple illustration may give a clearer picture of the general idea involved here. Let us consider any one of the deflection methods for evaluating e/m . No one of these methods involves the value of the Faraday. Let us then calculate the weighted average value of e/m , from all such investigations, and *assume* it to be the *true* value. Now consider the *so-called* evaluation of e/m by means of the measured wave-lengths of corresponding $H\alpha$ and $D\alpha$ lines (as in reference 37). Such experimental material gives directly only a value of the *atomic weight* of the electron. To obtain e/m one must *assume* a value of the Faraday. But now we *reverse* the process. We assume that the value of e/m is already known, and we use the same experimental material to evaluate F . An alternative form of statement is as follows. What value of the Faraday is it necessary to assume in order to bring the average of all spectroscopic values of e/m that involve the Faraday into coincidence with the average of all deflection values of e/m , which do not?

In using the *general* method for an indirect evaluation of F , one can include *all* available experimental material. But certain experimental results, as for instance the Zeeman effect evaluation of e/m , are surprisingly discordant. One can therefore delete one or more of these suspicious results, and then get new averages. Space prevents me from going into details, and I merely report that the use of *all* available material leads to an indirect value of F that has a computed probable error of only 10 coul—just equal to that

²⁷ W. Hsueh, Phys. Rev. **67**, 66 (1945).

assigned to the voltmeter value—but that lies some 15 coul above the voltmeter value. On the other hand, the deletion of one or more suspicious observations leads to new weighted average values of F running as high as 30 or so coulombs above the voltmeter value. The computed probable error is roughly 10 coul in all cases.

We therefore conclude from our indirect evaluation of the Faraday that the voltmeter value, although it seems reliable, on *internal* evidence, to 10 coul or less, *may* be too *low* by as much as 30 coul. But it certainly *cannot* be in error by two or three *hundred* coulombs.

* * *

The foregoing discussion includes a summary of the experimental material on the oil-drop value of the electronic charge. The best experimental values of the Faraday and of the Avogadro number have also been considered, since the ratio of these two quantities gives us an indirect, but at present the most trustworthy evaluation of e . There are many *other* indirect methods for evaluating e , but the only one that has been mentioned here is the value deduced from *all* experimental material *independent* of both F and N_0 . I believe, at the moment, that the true value of e probably lies between 4.802 and 4.803 $\times 10^{-10}$ abs-esu.

Atomic and Molecular Theory Since Bohr: Résumé of Specific Results

HENRY MARGENAU AND R. B. SETLOW
Yale University, New Haven, Connecticut

THE first two articles of the present series¹ were designed, respectively, to trace the genesis of the main ideas composing modern atomic and molecular theories, and to place them within a framework of logic. It would now seem desirable to round out the picture on the factual side. In view of this situation, our original plan conceived as the third and last contribution to this sequence an expository summary of all essential achievements in the field since Bohr's first triumph. The naiveté of this ambition should have been apparent at once, but it failed to impress itself upon the authors until the article at hand came to be written.

Many have succeeded in fusing ten textbooks into one; but to condense one hundred books into the space of twenty pages forboded certain failure. We were thus confronted with a choice between two alternatives: a mere listing of all significant results, or a selection of special and interesting items with attendant brief clarification. The latter course seemed simpler and more in keeping with the preceding material, and was adopted. It is hoped, however, that the selection of topics reflects not merely the authors' preference or individual interests; but illuminates typical specimens from the wide range of theories whose validity they exemplify. Certainly this has been the motive in the authors' choice.

1. SPECTROSCOPY

Atomic Spectra

One of the large stumbling blocks in the way of acceptance of the Bohr theory lay in its failure to explain quantitatively the spectra of polyelectronic atoms. This failure arose chiefly from the impossibility of assigning a definite orbit and sense of rotation to the electrons making up the atom. Out of this failure arose quantum mechanics. New advances were immediately made in correlating the multitude of facts of empirical spectroscopy.

Quantum mechanics retained the Bohr frequency condition $\Delta E = h\nu$, but departed from the older theory in the method of obtaining stationary energy states and selection rules. Thus the correlation of experimental facts with theory rested upon the solution of two distinct problems: first, the prediction of the stationary energy states; and second, the determination of selection rules whereby not all possible combinations were permitted. The first problem involves setting up the Schrödinger equation and finding those values of the energy that will give acceptable solutions. In the case of the hydrogen atom, it is possible to obtain an exact solution of the Schrödinger equation, but for atoms with more than one electron, the solution of this equation is not nearly so simple. If the wave equation for a relatively simple atom such as helium is set up, it is found to contain a term which involves the

¹ H. Margenau and A. Wightman, *Am. J. Phys.* **12**, 119, 247 (1944); these two preceding papers will hereafter be referred to as I and II, respectively.

relative distance r_{12} between the two electrons. Taking the origin at the nucleus, and letting r_1, θ_1, φ_1 be the coordinates of the first electron and r_2, θ_2, φ_2 , those of the second, we find that the Schrödinger equation takes the form

$$\frac{\hbar^2}{2m}(\nabla_1^2 + \nabla_2^2)\psi + (E + 2e^2/r_1 + 2e^2/r_2 - e^2/r_{12})\psi = 0. \quad (1)$$

Owing to the presence of the term containing r_{12} , it is not possible to separate the variables and hence obtain an exact solution. However, it is possible to solve the equation by approximate methods.

One method of attack is to start with a Schrödinger equation that does admit of an exact solution. Let such an equation be

$$H\psi_i = E_i\psi_i. \quad (2)$$

The actual Schrödinger equation involved is then written in the form

$$(H + H')(\psi_i + \varphi') = (E_i + \epsilon')(\psi_i + \varphi'), \quad (3)$$

where the primed quantities represent first-order corrections to ψ_i and E_i of the approximate Eq. (2). If Eq. (2) is subtracted from Eq. (3) and terms involving second-order corrections are discarded, there results

$$(H - E_i)\varphi' = (\epsilon' - H')\psi_i. \quad (4)$$

Now, making use of the fact that the functions ψ form a complete set, we have $\varphi' = \sum_j a_j \psi_j$, the a_j being undetermined constant coefficients. Substituting this expression into Eq. (4), multiplying both sides of the equation by ψ_k^* and integrating, we find the following result:

$$a_k E_k - a_k E_i = \epsilon' \int \psi_k^* \psi_i d\tau - \int \psi_k^* H' \psi_i d\tau. \quad (5)$$

Calling the last integral H'_{ki} , we have the two cases

$$\begin{aligned} k=i & \quad \epsilon' = H'_{ii}, \\ k \neq i & \quad a_k = H'_{ki}/(E_i - E_k). \end{aligned} \quad (6)$$

Since normalization of $\psi_i + \varphi'$ requires that $a_i = 0$, the first-order corrections of the energy E_i and state function ψ_i may be found by evaluating the integrals involved in Eq. (6). Higher order corrections are found in a similar fashion.

This theory is applicable to the ground state of helium. In this case the first-order correction of the Hamiltonian would be e^2/r_{12} . For if this term is omitted in Eq. (1), the variables may be separated and yield two hydrogen-atom equa-

TABLE I. Ionization potentials in electron volts.

	Observed	Calculated first-order correction	Hylleraas' approximation
He	24.463	20.30	24.465
Li ⁺	75.28	71.03	75.26
Be ⁺⁺	153.1	148.8	153.1
B ⁺⁺⁺	258.1	253.7	258.0
C ⁺⁺⁺⁺	389.9	385.6	390.0

tions whose solutions are well known. In Table I are given the observed values, together with the values calculated from the first-order correction of the ionization potentials (in electron volts) of several helium-like atoms. The computed values obtained by Hylleraas² with corrections beyond the first order are also shown.

The agreement between the first-order correction and the observed values is ample proof of the applicability of quantum mechanics to problems of this type. The results of Hylleraas add the finishing touch. Note that as the nuclear charge increases, the discrepancy between the first two columns decreases. This is to be expected since the term in the Hamiltonian representing the interaction of the two electrons becomes smaller compared to the energy of the electrons due to nuclear attraction. Another method of attack on this problem is outlined in SEC. 2.

The determination of the energy levels of atoms with more than two electrons presents a more difficult problem than that of helium-like atoms. Thus we find that the theoretical determination of the energy levels for most polyelectronic atoms has not been carried out. However, in the cases in which a solution has been worked through, the agreement between theory and experiment is striking.

The second problem concerning atomic spectra, namely, the determination of selection rules, does admit of a solution even in the case of atoms with many electrons. At first sight this problem seems, if anything, more difficult than the determination of energy levels. It is not necessary to determine the state function ψ to find the energy, but a knowledge of it would seem necessary to determine selection rules since the rules depend upon integrals of the type,

$$\int \psi_j^* (\sum_i e_i q_i) \psi_k d\tau, \quad (7)$$

where ψ_j and ψ_k are the state functions corre-

² Hylleraas, *Zeits. f. Physik* **54**, 347 (1929).

sponding to the energy levels E_j and E_k , and q_i represents the coordinates of the i th particle with charge e_i . If this integral equals zero, the transition from the level of energy E_j to one of energy E_k is forbidden for ordinary (dipole) radiation.

Prior to 1928, physicists had developed a more or less empirical scheme to govern the possibilities of transitions between various energy levels. These selection rules were part of a scheme known as the "vector model." In 1928 Slater³ was able to show by a general consideration of the forms of state functions of various states that orbital, spin and total angular momenta behaved as in the vector model. Thus, the theoretical basis for the vector model was established. Moreover, Wigner⁴ and Weyl⁵ derived the selection rules of the vector model on the basis of group theory. A detailed analysis of this sort is outside the scope of this report. Suffice it to say that in deriving the selection rules, the actual form of the wave functions does not have to be known. All that is needed is a knowledge of the symmetry characteristics of the various atomic states.

The picture of the atom as presented by the vector model was one consisting of rotating electrons revolving about a central nucleus. Associated with each electron were three quantum numbers. The integral quantum number n specified the "shell" in which the electron was located; it was the value of n that chiefly determined the electron's energy. The integral quantum number l gave the angular momentum of the electron about the nucleus in units of \hbar , while the spin quantum number s [$=\pm\frac{1}{2}$] gave the angular momentum of the electron about its own axis in units of \hbar . Electrons with the same total quantum numbers but with different values of l and s differed slightly in energy. For weak interactions between the spins and orbital motion, it was considered that all the orbital quantum numbers added up vectorially to give a resultant L , also an integer, while the spins of the electrons added up to a resultant S for the atom which was either integral or half integral, according as the number of electrons was even or odd. Furthermore, the resultant values for the atom, L and S , were added to give a total angular momentum J which was either integral or half integral, as S was. Thus, by specifying the quantum numbers of

each electron and their resultants, the state of an atom could be fixed.

In order to limit the number of combinations of states which would result in the emission of dipole radiation, it was necessary to restrict the changes which L and J could undergo. Thus L had to change from an even to an odd value or vice versa, while J could change by ± 1 or 0 (the transition from $J=0$ to $J=0$ was excluded). In terms of these selection rules it was found possible to classify and correlate the empirical knowledge of atomic spectra into one coherent body.

The multiplet structure of spectral lines was explained by the dependence of the quantum number J upon the resultant spin S . As an example, let us consider the principal series of sodium. We note that, for the closed shell, the total angular and spin momenta are both equal to zero. In the ground state the outer electron has quantum numbers $l=0$, $s=\pm\frac{1}{2}$. Thus, in view of the properties of the closed shell, $L=0$, $S=\frac{1}{2}$, and the total angular momentum J is $\frac{1}{2}$. In the first excited state, on the other hand, $l=1$. Thus $L=1$, $S=\frac{1}{2}$ and $J=\frac{1}{2}, \frac{3}{2}$. These two values of J indicate that the first excited level is split in two. Thus the transition from this split level to the ground state gives rise to the sodium D lines, a well-known doublet. Owing to a similar splitting in the higher levels, the principal series results.

As another example we may consider the case of an atom with two electrons outside a closed shell. The total spin of the atom may take the values 0 or 1. Thus for each value of L (other than $L=0$), J may have the values $L+1$, L or $L-1$. As a result, these levels split into three parts, giving rise to triplet spectra. Higher multiplet spectra are explained in a similar manner.

Molecular Spectra

The subject of molecular spectra as we know it today first appeared with the advent of the Bohr theory. The observed spectra were separated into their rotational, vibrational and electronic components, thus simplifying the analysis of such spectra. The neatness of this point of view was in a large measure responsible for the advances made in this field. Application of the Bohr theory to the rotation-vibration spectra of simple diatomic molecules yielded good agreement between theory and experiment. However, in the case of the electronic spectra the theory was powerless.

With the appearance of quantum mechanics other important advances were made in corre-

³ Phys. Rev. **34**, 1293 (1929).

⁴ *Gruppentheorie und ihre Anwendung auf die Quantenmechanik der Atomspektren* (Vieweg, Braunschweig, 1931).

⁵ *The theory of groups and quantum mechanics* (Methuen, 1931).

lating theory and experiment. For example, the integral rotational quantum number J of the older theory was replaced by $J + \frac{1}{2}$ in the new, a change that resulted in better agreement with observations. The same change took place with regard to the vibration quantum numbers. Moreover, the analysis of the electronic part of molecular spectra was now possible.

In the analysis of the infra-red spectrum of a diatomic molecule, it is necessary to consider both rotation and vibration of the molecule. For a first approximation it is sufficient to look on the molecule as a rigid rotator executing simple harmonic vibrations. However, it is immediately obvious that such a model is very inadequate. First, since the molecule is vibrating, it cannot be a rigid rotator; and second, if the molecule is to have a finite dissociation energy, it cannot be a harmonic oscillator. Thus we are led to regard the molecule as a nonrigid rotator executing anharmonic oscillations. Moreover, since the configuration of the molecule depends upon its vibrational state, we would expect the rotational energy levels to be affected by the vibrational state. In other words, coupling exists between rotation and vibration. A molecule conceived of as a nonrigid rotator has rotational energy levels that are depressed relative to those of the ideal case, the depression increasing with higher quantum numbers. A similar result is obtained for the anharmonic vibrator. A solution of the Schrödinger equation for the nonrigid anharmonic vibrating rotator yields values for the energy levels of the form

$$E = \omega_e(v + \frac{1}{2}) - \omega_e x_e(v + \frac{1}{2})^2 + \dots + B_v(J + \frac{1}{2})^2 - D_v(J + \frac{1}{2})^4 + \dots,$$

where v and J are the vibrational and rotational quantum numbers. The constants ω_e and x_e are intimately related to the forces between the atoms, while B_v and D_v are rotational constants depending upon the atomic masses, the internuclear distance and the quantum number v . By measurement of the band heads and fine structure it is possible to arrive at values for these constants. The knowledge thus obtained of the force constants and internuclear distances is of critical value to an understanding of interatomic forces (see SEC. 2). It should be mentioned that the prediction of the spectral properties of deuterides from those of the corresponding hydrides furnishes an excellent verification of the theory.

In recent years much work has been done on the vibration-rotation spectrum of polyatomic

molecules. While relatively few cases have been analyzed completely, those which have provide vital information concerning molecular structure. The application of the theory of groups has immensely simplified the mathematical treatment of these molecules.⁶

The electronic energies of molecules was a problem far beyond the scope of the Bohr theory. Quantum mechanics is able to solve these problems, but only after overcoming great mathematical difficulties. Several methods of attack will be outlined in SEC. 2.⁷ The general quantum mechanical results of these investigations are compactly summarized in a molecular vector model. We shall not treat this model except to mention that of necessity it is more complex than the atomic one owing to the various types of coupling that may exist between electronic and nuclear motion. A good description of the vector model for molecules and its uses is given by Herzberg.⁸

As in the case of atomic spectra, it is possible to derive selection rules governing the transition of a molecule from one energy state to another. In these derivations the symmetry characteristics of the molecule and its wave function play an important role. We shall content ourselves with a discussion of just one of the many selection rules: for diatomic molecules with identical nuclei, the transition between symmetric and antisymmetric states is forbidden. The terminology "symmetric and antisymmetric states" refers to the fact that the total wave function of the molecule either remains unaltered or changes sign upon interchange of the two nuclei. The derivation of this rule runs as follows. The probability of a transition from a state of wave function ψ_i to one of wave function ψ_k with the emission of dipole radiation depends on an integral of the form (7). If ψ_i is symmetric and ψ_k antisymmetric, then the integrand will change sign upon interchange of the nuclei, since $\sum_i e_i q_i$ does not change sign under such a transformation. Thus by interchanging two like nuclei, we have changed the sign of the integral. But since the value of the integral cannot depend upon the manner of designating the nuclei, the positive value of the integral must equal the negative value. Thus the

⁶ D. M. Dennison, *Rev. Mod. Phys.* **12**, 175 (1940); J. Rosenthal and G. M. Murphy, *Rev. Mod. Phys.* **8**, 317 (1936).

⁷ See also H. Sponer and E. Teller, *Rev. Mod. Phys.* **13**, 75 (1941).

⁸ *Molecular spectra and molecular structure, diatomic molecules* (Prentice Hall, 1939), p. 231.

integral is identically equal to zero, and the transition from a symmetric to an antisymmetric state (and vice versa) is forbidden. Group theory employs this type of reasoning in its derivation of selection rules for more complicated molecules.

The foregoing selection rule holds rigorously so long as we neglect nuclear spin.⁹ If the two nuclei have opposite spins, then they are no longer identical and the proof does not hold. However, even in this case the probability of the transition, symmetric to antisymmetric, is extremely small. If for the moment we neglect the effect of nuclear spin, we reach the following conclusion. If all the molecules of oxygen were once in symmetric states, they will always be in symmetric states, and similarly for antisymmetric states. Now the symmetry characteristics of energy levels alternate. Thus every other level would be inaccessible to an O₂ molecule. We would expect every second line to be missing from the electronic band spectra and also from the Raman rotational spectrum. This is actually observed for O₂, as well as for C₂ and He₂. Owing to the effect of the nuclear spin, we are led to suspect that some of these missing lines might appear even though with reduced intensity. They do not. The conclusion is that the nuclei of O, C, He and several other kinds of atoms have zero nuclear spin.

In the case of H₂, we have two possibilities, since the nuclear spin is $\frac{1}{2}$. The total nuclear spin of the molecules is either 1 or 0, corresponding to the well-known ortho- and para-modifications. In an applied magnetic field the ortho-modification with total nuclear spin 1 will have its degeneracy removed so that the components of the nuclear spin along the magnetic field may be 1, 0, -1, while the para-modification may only have the component 0. Thus the statistical weight of the ortho-modification is three times that of the para. As a result, we should expect alternate rotational lines to have an intensity ratio of 3:1. This is observed and is taken as evidence that a proton has a nuclear spin of $\frac{1}{2}$. In general, if the nuclear spin of an atom is I , the ratio R of the intensities of alternate lines is given by

$$R = (I+1)/I.$$

Thus a measurement of intensity ratios yields nuclear spins. This is probably the simplest method of determining these quantities. Their importance in nuclear physics will be discussed in

SEC. 5. It should also be mentioned that the symmetry characteristics of molecular energy levels for molecules with similar nuclei enable one to determine the type of statistics, Bose-Einstein or Fermi-Dirac (SEC. 3), which the various nuclei follow.

2. INTRA- AND INTERMOLECULAR FORCES

The fact that atoms attract one another to form aggregates is a recognition as old as atomic theory itself. While Empedocles accounted for such adhesive and repulsive tendencies vaguely by reference to love and hatred, and Democritus (according to later writers) equipped some of his atoms with interlocking mechanisms, later chemists renounced all pretense to understanding and humbly spoke of *valence forces*. Prior to the period covered in this report it was generally agreed that these forces must be of electric origin, and fruitful attempts were made, chiefly by Lewis and Langmuir, to link them with the electronic structure of matter. Qualitatively, two types of "bonding" could be recognized: that between ions, as in the formation of NaCl, and that between neutral—and indeed spherical—atoms as in the formation of H₂. The latter, often called *homopolar bonding*, remained inexplicable even when Bohr's picture of the electrical atom became known.

As compared with other forces, the valence force presented two rather striking aspects. First, it appeared to be of very small range; that is, two atoms practically have to come in contact before valence action is called into play. Second, the force can be saturated; having been activated a specific number of times (equal to the valence of the atoms), it ceases to exist. The only classical mechanism which possesses these two characteristics is the interaction between permanent multipoles, that is, neutral structures carrying a rigid framework of electric charges. It was natural, therefore, that the explanation of bonding was sought in this picture. Some valence actions thus did yield to understanding, but the homopolar bond, notably the simple union of two hydrogen atoms, remained as mysterious as ever.

Quantum theory with its principle of exclusion (see paper II, SEC. 3) illuminated the entire field. It was able to give a detailed account of what happens in all valence actions. Moreover, it provided a vantage point from which all types of chemical forces appear related and many time-honored distinctions fail; reactions previously considered to be "ionic" are now seen to be partly

⁹ Like the electron (see papers I; II SEC. 4) the atomic nucleus has been found to possess an angular momentum, called *spin*, and a magnetic moment.

"ionic" and partly "homopolar," or the reverse may be true. Since in the present section we wish to describe this new situation, it seems well to start out with a clarification of the terminology that is now accepted by chemists. In this we follow Pauling's account.¹⁰

Distinction is made among three types of bond: the electrostatic, the covalent, and the metallic. The first includes the old ionic bond but also the union of multipoles. The complex $[\text{Fe}(\text{H}_2\text{O})_6]^{+++}$, for example, is held together largely by electric forces between the Fe ion and the dipoles and quadrupoles of the surrounding water molecules. While not ionic, the structure is classified as illustrating the electrostatic bond. Covalency is a more specific name for homopolar union and is exemplified by all molecules formed from identical atoms. Crystals, too, are often held together by valence forces and should therefore be regarded as supermolecules. Metallic crystals, in particular, owe their cohesion to forces of much the same type as H_2 , hence the third type in our list.

The metallic bond is interesting enough in its own right to be reserved for special consideration, together with other properties of metals, in the next section. Electrostatic interaction has not been appreciably affected by modern theory and may therefore be omitted from our review. The remainder of the present section, so far as it treats of intermolecular forces, will therefore deal chiefly with the problem of the covalent bond. We shall present its simplest features in connection with the H_2^+ ion and the H_2 molecule and then comment briefly on a general principle relating to all types of bond, the principle of resonance.

One-Electron Bond

Covalent bonds may be subdivided according to the number of electrons which the united atoms share. The simplest example of a one-electron bond is to be found in the formation of H_2^+ from a hydrogen atom and a proton.

Whenever it is desired to determine the forces between two atomic or molecular complexes, one must first find the total energy of the pair. If the total differs from the sum of the energies of the two partners at infinite separation (that is, in the state of effective isolation), the two are said to possess an energy of interaction, or a mutual potential energy, equal to the difference. If,

furthermore, this difference is a function of the distance of separation between the partners, forces are present; the magnitude and direction of these forces is given by the derivative of the interaction energy with respect to the distance of separation.

In order to find the total energy of the pair it is necessary to solve the Schrödinger equation. In the present example the moving particles are two protons and one electron. But the three-body problem is difficult to solve, and since the electron is much less massive than a proton, it seems obvious that a good approximation may be obtained by treating the two protons as fixed centers of force, a distance R apart, and allowing the mobile electron that energy state which, in accordance with the Schrödinger equation, it will occupy in this field of force.

Mathematically the problem can be solved exactly in this form, and the solution leads to two interesting results. The first concerns the energy. It is found that E_0 , the lowest among all eigenvalues of the energy, is indeed a function of R and possesses a minimum at precisely that interprotonic distance R_0 which corresponds to the empirical distance in the ion. Since the minimum corresponds to the equilibrium distance, the quantum mechanical explanation for the union of H and H^+ is satisfactory in this respect. But it is to be noted further that the value of $-E_0$ at the minimum agrees with the experimental binding energy of the ion.

The second result concerns the state function corresponding to E_0 . Its square, which represents the probability connected with the electron's position,¹¹ is large in the region between the protons and very small beyond. The electron may therefore be said to reside largely between the two positive charges which would otherwise repel each other, and to effect their cohesion. It is as though the electron formed a cement of negative charge holding the two protons together.

While there is at first sight nothing particularly striking about these results, we should remark that if the energy between H and H^+ had been calculated on the basis of the Bohr theory and not as a solution of the Schrödinger equation, the minimum could not be accounted for.

There are relatively few problems illustrating the action of covalent forces that can be solved exactly. Several approximation methods are then available, one of which, the Heitler-London method, we wish briefly to consider here, for it

¹⁰ L. Pauling, *The nature of the chemical bond* (Cornell Univ. Press, 1940).

¹¹ See papers I and II, Sec. 1.

furnishes the concepts and the terminology largely dominating the field of quantum chemistry. Its workings are most easily understood in connection with the H_2^+ problem we have just discussed.

If H is the Hamiltonian operator¹² of a given physical system, and ψ the normalized solution of the Schrödinger equation, $H\psi = E\psi$, then obviously

$$E = \int \psi^* H \psi d\tau.$$

Suppose now that because of mathematical difficulties an exact solution ψ cannot be found. It is frequently possible on physical grounds to find a function φ that has all the earmarks of being a good approximation to ψ . The corresponding energy, which will presumably be a good approximation to E , is then given by

$$E = \int \varphi^* H \varphi d\tau. \quad (1)$$

It may even happen that several different but, judged on physical grounds, equally good approximations to ψ are available. If they are called $\varphi_1, \varphi_2, \dots$, the best approximation to ψ must be of the form $a_1\varphi_1 + a_2\varphi_2 + \dots$ with constant coefficients a_1, a_2, \dots , as follows from the fact that the Schrödinger equation is linear and therefore permits a superposition of solutions.

In the case of H_2^+ , we know the correct solution in one extreme situation, namely, that in which one of the protons is at infinity. For then the electron state is exactly the ground state of a hydrogen atom. Thus, when the electron is associated with proton A , and proton B is far away, the state function is u_A , a hydrogen eigenfunction centered about A . This we may call φ_1 . But an equally good approximation is provided by u_B , which corresponds to the electron's attachment to proton B while A is far away. Calling this φ_2 , we have arrived at the conclusion that

$$\varphi = a_1 u_A + a_2 u_B \quad (2)$$

is a reasonable state function so long as the protons are separated sufficiently far. The symmetry of the problem further leads us to assume $a_1 = a_2 = a$. The hazard of the method, which is compensated by its success, arises from the further supposition that φ is still a fair approximation when the protons are separated by a finite distance.

If we wish to find the energy of the ion, we

must substitute φ from Eq. (2) into Eq. (1). This, however, requires knowledge of the Hamiltonian H . But this is nothing other than

$$H = H_A - e^2/r_B + e^2/R,$$

where H_A is written for the Hamiltonian of a hydrogen atom whose state function is u_A , and r_B denotes the distance of the electron from nucleus B . If the reader will make the substitution and remember a few of the properties of the functions u_A and u_B , he will obtain

$$E = E_H + \frac{e^2}{R} + \frac{J+K}{1+\Delta}, \quad (3)$$

with

$$J = - \int u_B^2 \frac{e^2}{r_A} d\tau, \quad K = - \int u_A u_B \frac{e^2}{r_A} d\tau$$

and

$$\Delta = \int u_A u_B d\tau.$$

When these integrals are evaluated, the result is a value E which depends on R parametrically in the manner already described, yielding a minimum at a distance near the experimental equilibrium position—though differing somewhat because of the defects of the approximation.

Had we omitted the function u_B from Eq. (2), the result would have been

$$E = E_H + \frac{e^2}{R} + J,$$

and this would show no minimum. The stability of the ion therefore is critically dependent on the inclusion of both u_A and u_B , a fact that is of paramount importance. Its physical significance may be stated as follows. State u_A implies attachment of the electron to proton A , and u_B , attachment to proton B . Equation (2) then represents attachment with equal probability to A and B , that is, simultaneous occurrence of both conditions. The matter is put in a more plausible way when the functions u_A and u_B are written in their time dependence,¹³ for it then appears that the electron *oscillates* in time between these two conditions. This phenomenon is currently known as electron exchange or, more recently in the chemical literature, as *resonance*. Stability may be said to arise from resonance of the electron between two states.

Equation (3) further confirms this point. It makes the quantity K responsible for the electron bond. Looking more closely at its anatomy we

¹² See paper II, Sec. 1.

¹³ See paper II, Sec. 2.

find it to be a monstrosity, for it represents an average of the Coulomb energy $-e^2/r_A$, taken not over the probability distribution u_A^2 or u_B^2 , but over the product $u_A u_B$, which in general does not signify a probability at all. It is as though the Coulomb energy $-e^2/r_A$ were multiplied by an exchange operator¹⁴ P_{AB} and had taken on the symbolic form $U = -(e^2/r_A)P_{AB}$ before the integral $K = \int u_A U u_A d\tau$ could be computed. For this reason K is called an *exchange integral*. The presence of the covalent bond is always associated, mathematically, with the occurrence of exchange integrals of this kind.

Since K has no classical analog, it is difficult to form a simple picture of the mechanism of a covalent bond. Also, it is true that the idea of electron exchange is entirely artificial, arising from an incidental approximation scheme with no further claim to mathematical significance than success. Exchange integrals would not occur if the Schrödinger equation were solved exactly, as by numerical methods. Nevertheless, their methodological function and their convenience as stepping stones to approximate solutions are so great that the literature has accepted them as important elements in its theoretical apparatus and its nomenclature.

Two-Electron Bond

Covalent bonding in the hydrogen molecule takes place through the sharing of two electrons by the protons A and B . The Heitler-London method will again be applied to account for the valence force, although in this instance, too, more accurate though cumbersome procedures are available. Since it now becomes necessary to distinguish between the electrons as well as between the protons, we shall label the former 1 and 2, while continuing to designate the latter by A and B . When two hydrogen atoms are sufficiently far apart their state function is simply $u_A(1)u_B(2)$, in accordance with the considerations of paper II, SEC. 3. This implies that electron 1 is attached to proton A , electron 2 to proton B . But so far as the physical situation is concerned, the attachment might well be the converse, corresponding to the function $u_B(1)u_A(2)$. Repeating a former conjecture, we assume that

$$\varphi = a_1 u_A(1)u_B(2) + a_2 u_B(1)u_A(2), \quad (4)$$

is a satisfactory approximation to ψ . Symmetry again indicates equality of a_1 and a_2 , though here

somewhat closer reasoning is required. The proportion in which the two conditions are mixed in φ is not a_1 to a_2 , but $|a_1|^2$ to $|a_2|^2$. All we may stipulate, therefore, is that $a_2 = \pm a_1$. But here the Pauli principle¹⁵ enters decisively. The state function φ must be antisymmetric with respect to an exchange of the two electrons, and if Eq. (4) told the whole story we should conclude that $a_2 = -a_1$. In fact, however, Eq. (4) is incomplete because it ignores the spin coordinates, to which very little attention was paid in the previous articles. Suffice it to say, therefore, that the omitted part of the function, which appears as a factor of φ as written, is known (from the magnetic behavior of the molecule) to be antisymmetric: if 1 and 2 are interchanged in Eq. (4), this alone will cause the function to change sign. The remainder must therefore be symmetric; hence $a_2 = +a_1$.

When Eq. (4) is substituted in Eq. (1) with

$$H = H_A + H_B - e^2(r_{A2}^{-1} + r_{B1}^{-1} - r_{12}^{-1} - R^{-1}),$$

this being the operator which represents the energy of electrons 1 and 2 near nuclei A and B , the result turns out to be

$$E = 2E_H + \frac{e^2}{R} + \frac{2J + J' + 2K\Delta + K'}{1 + \Delta^2}. \quad (5)$$

Here Δ , J and K are the symbols previously defined, while J' and K' are new integrals typical of the two-electron problem:

$$J' = \int u_A^2(1)u_B^2(2) \frac{e^2}{r_{12}} d\tau_1 d\tau_2,$$

$$K' = \int u_A(1)u_B(1) \frac{e^2}{r_{12}} u_A(2)u_B(2) d\tau_1 d\tau_2.$$

When E of Eq. (5) is plotted as a function of R , the curve is seen to have a minimum in the neighborhood of the experimental internuclear distance of H_2 . At this minimum, E is negative and equal in order of magnitude to the dissociation energy of the molecule.

Again, had one of the two constituents of φ [in Eq. (4)] been ignored, the result for E would have been Eq. (5) with K and K' put equal to zero. As a function of R , this E would show a very slight minimum at approximately the correct place, but far too shallow to account for the binding of the molecule. Responsibility for the bond is therefore borne, as before, by the *exchange integral*, which in this case appears in two

¹⁴ See paper II, SEC. 3.

¹⁵ Paper II, SEC. 3.

different forms

$$K = \int u_A(1) \left(-\frac{e^2}{r_{A1}} P_{AB} \right) u_A(1) d\tau_1,$$

$$K' = \int u_A(1) u_B(2) \left(-\frac{e^2}{r_{12}} P_{AB} \right) u_A(1) u_B(2) d\tau_1 d\tau_2.$$

As in the previous instance of the one-electron bond, the form of φ indicates that the state of each electron alternates between attachment to proton *A* and to *B*. The two-electron bond is therefore also due to *resonance* between two states.¹⁶ Finally, we note that if φ^2 were mapped as a function of position in space, its values would be largest between the protons.¹⁷ It may therefore be said that the bond is formed by a "piling up" of negative charge between the two protons.

Leaving aside the strangeness of the mathematical apparatus which has led to these results, one cannot help being thoroughly amazed at the remarkable way in which modern quantum mechanics has justified and substantiated the ingenious guesses of pre-quantum chemists. G. N. Lewis, in particular, had explained the covalent bond as due to the sharing of one or more electrons between atoms, thus anticipating in a sense the results of modern theory.

Bonding may involve more than two electrons; the procedures designed to deal with such complex situations are extensions of those here illustrated and do not call for special comment. In general, each valence line drawn by chemists to represent a covalent bond signifies an electron shared between the atoms. Moreover, the phenomenon of *directed valence* lends meaning to the chemist's habit of drawing these lines at definite angles; directionality is imparted to the scheme by the nature of the electron's atomic orbit. Thus if the bonding electron is an *s*-electron (having no orbital angular momentum) it will show no preference as to direction. However, *p*-electrons (having one unit of angular momentum) may be shown to form bonds along three mutually perpendicular directions, a fact borne out by the known structure of many molecules, for instance, water.

¹⁶ The reader will observe that the word "state" is here used in a loose sense not conformable to the usage in the previous articles. The axioms of paper II would forbid us to speak of a state except in relation to the entire system of protons and electrons.

¹⁷ If the reader will do this he may not find the argument quite convincing, for the Heitler-London method operates with a state function which yields a rather poor approximation to the probability distribution in space. More accurate state functions would, however, bear out this point.

In one respect the modern view is somewhat confusing, for it wipes out distinctions of long standing. Even the covalent bond is conceptually dissolved into a great number of types. It owes its existence to "resonance" between different electronic states. In a similar way, it is possible for bonds to be formed by resonance between all sorts of states, some perhaps ionic, others those corresponding more nearly to covalency. The idea of resonance mixes, as it were, all types of binding and makes the grouping discussed at the beginning of this section in principle illusory. Its retention is only a matter of convenience.

Van der Waals Forces

Different from the valence forces of the preceding sections are the so-called van der Waals forces, chiefly because of their longer range and, roughly speaking, their additivity (failure to be saturable). Their name recalls the equation of state of a real gas, which differs from an ideal one by the presence, between the molecules, of the forces in question. They are also (and more properly) called *intermolecular* forces in contradistinction to the *intramolecular* forces of the valence type. This demarcation, too, is without complete justification, for chemists know of loosely bound molecules such as (O₂)₂, which are held together by van der Waals forces.

Aside from the deviations of real gases from ideal behavior, such phenomena as condensation, adhesion, capillarity, viscosity, the Joule-Thomson effect, broadening of spectral lines, and innumerable others are caused by van der Waals forces. In spite of their ubiquity, their origin was not completely understood until the advent of quantum mechanics. Prior to 1928, they were believed to be the effect of the interaction of multipoles; molecules known to possess no dipole moment were assumed to carry quadrupoles which, by inducing dipoles in their partners, attracted them. Here the field of speculation was wide and uncontrolled, for quadrupole moments of molecules cannot be measured directly. But a blow was dealt this sort of reasoning when Schrödinger's theory showed without ambiguity that some atoms, like rare gases, which do exhibit van der Waals forces, cannot possess quadrupole moments. Clearly, then, there must be at least one type of intermolecular force not traceable to classical, electrostatic models.

Here the new theory, after uncovering the shortcomings of the old, soon provided an explanation. The origin of the forces was found, chiefly

by F. London, in an interesting mechanism previously unrecognized. To form a simple picture of it, consider the motion of the two electrons in a helium atom, which electrons, for the sake of the present discussion, we may regard as amalgamated into one cluster of charge. According to quantum theory, the average position of this cluster as to longitude and latitude about the nucleus, called its "phase," is random since the state function has spherical symmetry. This situation corresponds to the classical picture of a negative charge moving rapidly about the positive nucleus, not about an axis, but on the surface of a sphere all points of which are reached with equal probability. Thus, while the atom at any *instant* represents an electric dipole, quadrupole, and so on, its *average* behavior is entirely nonpolar.

Suppose now that another atom is brought into the neighborhood of the first. If its cluster of charge moved in the same random manner, the instantaneous poles of the two would cause rapid and random alternations of attractions and repulsions, and the residual effect would be nil. However, by virtue of the electrostatic field which it generates, the instantaneous polarity of the first atom will cause the cluster of charge in the second to move more or less in harmony with the first. The closer the atoms, the more perfect will be this phase agreement. Now two dipoles or quadrupoles whose axes are parallel always attract, and this attraction accounts for the intermolecular force between so-called nonpolar atoms and molecules.

An effect of similar nature occurs when a light wave passes over a molecule: the electric field of the wave then has the tendency to pull the cluster of charge into phase agreement, causing the well-known phenomenon of light dispersion. For this reason, London has suggested the name "dispersion force" for the attraction just considered. Calculation shows this force to have the known range of intermolecular attractions, to be attractive in all simple instances, and to fall off with distance of separation R like the series $a/R^7 + b/R^9 + c/R^{11} + \dots$. For large values of R , only the first term in this series is of importance.

For atoms that exhibit no valence forces, such as He, the exchange force calculated by the method of the previous paragraphs has no equilibrium distance R_0 , but is strongly repulsive in the region where R is smaller than the diameter of the atom. Thus arises the rigidity of atoms and molecules. If these repulsive forces are compounded with the van der Waals forces, the result

accounts satisfactorily for all known phenomena pertaining to molecular interactions.

In addition to the attractions here discussed, the older classical mechanism of electrostatic forces between multipoles remains in good standing wherever the structure of the molecule calls for it. With strongly polar substances such as water it is by far the more important.

3. METALS

Prior to the quantum theory, the picture which accounted most adequately for the unique properties of metals was due to Drude, who regarded a metal as a lattice of atomic ions surrounded by a lattice-pervading gas of free electrons. His theory, developed with greater refinement by Lorentz, led to a reasonable understanding of electric and thermal conductivities and of various thermoelectric effects. Its minor shortcomings were, first, its failure to yield proper values for the specific heat of metals—to explain, for example, the Dulong-Petit law. A diagnosis of this difficulty indicated that the contribution of the electron gas to the specific heat was too large. Second, it yielded an incorrect dependence of conductivity on temperature—unless quite arbitrary assumptions were made about the electron's mean free path. Finally, the theory predicted values for the Peltier and Thomson coefficients that were much larger than those measured.

Quantum mechanics added to these discrepancies a logical one of a major sort. Drude and Lorentz had used in their calculations Maxwell-Boltzmann statistics, familiar from classical statistical mechanics. According to Maxwell and Boltzmann, the number of electrons in the metal having kinetic energy ϵ is given by

$$n(\epsilon) = ce^{-\epsilon/kT}. \quad (1)$$

But Pauli was able to show that the electrons in a metal, because of their extreme concentration and small mass, could not follow this distribution law; their distribution should conform to a new type of quantum statistics proposed by Fermi and Dirac and should be described by the law

$$n(\epsilon) = \frac{c}{\exp \frac{\epsilon - \epsilon_0}{kT} + 1}, \quad (2)$$

in which c and ϵ_0 are constants depending parametrically on T . Sommerfeld proceeded to face the dilemma; he removed the logical objec-

tion by applying Eq. (2) in straightforward fashion to the analysis of Lorentz and found that most of the factual difficulties mentioned in the preceding paragraph disappeared together with the conceptual one. While the analysis achieving these results is lengthy, its highlights can be seen in the simple reasoning of the next two sub-sections.

Fermi-Dirac Statistics

To derive distribution laws (1) and (2) we consider an assemblage of electrons in thermal equilibrium. Although their kinetic energies will in general have a continuous range of values, let us divide this range into cells $\epsilon_1, \epsilon_2, \dots, \epsilon_i, \dots$. If n_i electrons have energy ϵ_i , the total energy of the assemblage will be

$$\mathcal{E} = \sum_i n_i \epsilon_i. \quad (3)$$

This total energy must, of course, be constant, although the assignment of the individual ϵ_i to specific electrons may change as a result of collisions. Suppose that one electron makes a transition from ϵ_r to $\epsilon_{r'}$, in a small time dt . This change alone would not conserve \mathcal{E} ; hence at least two electrons must change their energies. Let these changes be from ϵ_r to $\epsilon_{r'}$ for the one, and from ϵ_s to $\epsilon_{s'}$ for the other. To leave Eq. (3) intact, we must have

$$\epsilon_r + \epsilon_s = \epsilon_{r'} + \epsilon_{s'}. \quad (4)$$

Each such pair of changes has associated with it a certain probability, p , per unit time. The number of transitions $\left(\begin{smallmatrix} r \rightarrow r' \\ s \rightarrow s' \end{smallmatrix} \right)$ taking place in time dt will be proportional aside from p , to n_r, n_s and finally to dt . But this is not all, for the number may also depend on the number of systems present in the final states, that is, on $n_{r'}$ and $n_{s'}$. Clearly, if the electrons satisfy an exclusion principle, the number of transitions is zero if either of the final states is already occupied. We represent this dependence on the final state by $f(n_{r'})$ and $f(n_{s'})$ and note that, if the exclusion principle were inactive, we should have $f(n) = 1$. However, the exclusion principle causes f to vanish when the cell is occupied, that is, when $n = 1$ (supposing that there can be at most one electron per cell). But when $n = 0, f = 1$. Thus we may put $f(n) = 1 - n$.

The number of transitions in question will be proportional to

$$n_r n_s f(n_{r'}) f(n_{s'}) dt.$$

The type of transition thus far considered, $\left(\begin{smallmatrix} r \rightarrow r' \\ s \rightarrow s' \end{smallmatrix} \right)$, brings about a decrease in n_r . The reverse change, $\left(\begin{smallmatrix} r' \rightarrow r \\ s' \rightarrow s \end{smallmatrix} \right)$, which occurs (aside from a constant factor)

$$n_{r'} n_{s'} f(n_r) f(n_s) dt$$

times during the interval, will cause an increase in n_r . Since, in thermal equilibrium, increase and decrease must be equal,

$$n_r n_s f(n_{r'}) f(n_{s'}) = n_{r'} n_{s'} f(n_r) f(n_s),$$

or

$$\frac{n_r}{f(n_r)} \cdot \frac{n_s}{f(n_s)} = \frac{n_{r'}}{f(n_{r'})} \cdot \frac{n_{s'}}{f(n_{s'})}. \quad (5)$$

Now the ratio $n_r/f(n_r)$ is a function only of the energy ϵ_r ; let us call it φ_r . On taking logarithms of Eq. (5) we have

$$\ln \varphi_r + \ln \varphi_s = \ln \varphi_{r'} + \ln \varphi_{s'}.$$

The only solution of this functional equation which is consistent with Eq. (4) is the linear relation

$$\ln \varphi_r = a - b\epsilon_r. \quad (6)$$

Since the quantity b must be positive, the minus sign is needed because φ_r may not be infinite for infinite ϵ_r . We have thus found the answer:

$$n_r/f(n_r) = e^{a-b\epsilon_r}. \quad (7)$$

In the classical case (exclusion principle ignored) $f(n_r) = 1$, and Eq. (7) is identical with Eq. (1) provided the constants a and b are properly identified. If, however, in view of the Pauli principle we put $f(n_r) = 1 - n_r$, and then solve Eq. (7) for n_r , we obtain

$$n_r = \frac{c}{\exp(-a + b\epsilon_r) + 1}.$$

This, with proper choice of constants, is Eq. (2).

Sommerfeld's Theory

The reader will recall that the Drude theory, based on Eq. (1), gave values for the specific heat that are too large. One of the chief corrections effected by the use of the Fermi-Dirac distribution function was a reduction of these values, as was noted in the beginning. How this comes about can be seen qualitatively by an inspection of Eqs. (1) and (2).

Specific heat is the rate of change of total

energy with temperature T ; the total energy is

$$\int_0^{\infty} w(\epsilon) n(\epsilon) d\epsilon,$$

where $w(\epsilon)$ is a weight factor equal to the number of energy states per unit range in ϵ . It is a slowly varying function of ϵ and may for present purposes be disregarded. We see, therefore, that if any part of the graph of $n(\epsilon)$ versus ϵ is independent of T , that part will contribute to the total energy but not to the specific heat. Equation (1) shows a strong exponential dependence on T for every value of ϵ ; all electrons, regardless of their energies, must therefore contribute to the specific heat.

Let us now examine Eq. (2). In the limit in which $T \rightarrow 0$ the graph of $n(\epsilon)$ versus ϵ is a straight line parallel to the ϵ -axis so long as $\epsilon < \epsilon_0$. At $\epsilon = \epsilon_0$, $n(\epsilon)$ drops to zero, where it remains for all larger values of ϵ . This may be interpreted by saying that all electrons occupy the lowest energy cells available. As T increases, the curve continues to have this general character, except that the drop is no longer abrupt and vertical but develops into a curved tail.

The region in which this drop comes can be easily computed. At $\epsilon = \epsilon_0$ the value of n is $\frac{1}{2}c$. Now the place at which n is some factor r times $\frac{1}{2}c$ is given by

$$\epsilon = \epsilon_0 + kT \ln \left(\frac{2}{r} - 1 \right).$$

The range in which n falls from, say, $0.9c$ to $0.1c$, corresponding to $r_1 = 1.8$ and $r_2 = 0.2$, is, therefore,

$$\Delta\epsilon = kT \left[\ln \left(\frac{2}{r_2} - 1 \right) - \ln \left(\frac{2}{r_1} - 1 \right) \right] = (2 \ln 9) kT.$$

It is thus seen that the range of ϵ in which n differs from the fixed value it possesses at $T=0$ and in which it is temperature-dependent, is only a few times kT , whereas, according to Eq. (1), the entire range—of order of magnitude ϵ_0 —is temperature-sensitive. Roughly, then, the Sommerfeld theory predicts a value for the electronic specific heat that is about kT/ϵ_0 times the Drude value.

As was shown, ϵ_0 is the limit to which the electron energy levels are filled at the absolute zero of temperatures in the Fermi-Dirac distribution. It is of course larger the larger the density of electrons in the metal and can be computed for different metals. Its value turns out to be

$$\frac{h^2}{2m} \left(\frac{3N}{8\pi} \right)^{\frac{1}{3}},$$

where m is the electron mass and N the density. If ϵ_0 were of the same order of magnitude as kT , or smaller, Sommerfeld's theory would yield the same specific heat as Drude's; in fact, however, it is much larger because of the smallness of m and the magnitude of N . For Cu, $\epsilon_0 \cong 7$ ev, while at room temperature, kT is only $1/30$ ev. The ratio kT/ϵ_0 is therefore of the order of $1/100$.

In the matter of conductivity, electric and thermal, the Drude-Lorentz theory was satisfactory. It might therefore be expected that Sommerfeld's innovation, which by the use of the Fermi-Dirac distribution function "freezes" most of the electrons in their energy states, would take insufficient account of the electron's mobility in electric and thermal fields. This, however, is not the case; for while the electrons are frozen with regard to their energy states, they are still free to move through the metal. Indeed, the new theory yields formulas for the two conductivities which differ only insignificantly from Drude's; it yields a value for the ratio of the conductivities (the Wiedemann-Franz ratio) that is in excellent agreement with experience for many metals.

Despite these various successes the free-electron theory is only the first step toward perfection, and modern formulations have gone far beyond it. In view of the limited sense in which metallic electrons are really free, the achievements of this early idea seem almost miraculous. This is even more true when we reflect that the theory presumes to describe the properties of all metals in terms of only two disposable parameters, the constants c and ϵ_0 in the distribution law (2).

The respects in which this description proves impossible require a more detailed picture, one conformable with the circumstance that the electrons are not free but move in a field of force due to the metallic ions. This picture has been rather fully developed under somewhat conflicting names, such as the "band approximation," "zone structure," or simply "electronic structure" of metals. A brief review of it will now be given.

Quantum Mechanical Theory of Metals

Developments of the last 10 years are dominated by two purposes: (i) to account for the conductive and thermoelectric properties of metals more adequately than the free-electron theories are able to do; (ii) to probe into the very structure, ionic and electronic, of the metal itself. The second purpose is quite foreign to all previous theories which, in an obvious sense, took the metal for granted. The new structure theories

explain the *cohesion* of metals, thereby forming a link with the material discussed in the preceding section. We first turn our attention to the refinements suggested by quantum mechanics in the description of the behavior of electrons in a metal.

Let us try to answer the question: To what extent can an electron, moving in the periodic field of an ionic lattice, be said to be free?

According to the principles of paper II, SEC. 1, the state of an electron moving in a field of potential $V(x)$ (for simplicity we assume one-dimensional motion) must satisfy the Schrödinger equation,

$$-\frac{\hbar^2}{2m} \frac{d^2\psi}{dx^2} + V\psi = E\psi. \quad (8)$$

In the present case, V is periodic in space; it repeats its form from ion to ion. Now a mathematical theorem due to Floquet assures us that the general bounded solution of Eq. (8) with periodic potential V has the form

$$\psi = c_1 e^{ikx} P(x) + c_2 e^{-ikx} P(-x), \quad (9)$$

where P is a periodic function of x having the same period as V . The reader will recall that $e^{\pm ikx}$ represents the state function of a free electron. We see, therefore, that the actual state of the electron is a compromise between freedom (the factor $e^{\pm ikx}$) and capture by an ion (the factor $P(\pm x)$). But Eq. (9) is quite noncommittal as to the matter of *which* ion has captured the electron; since P is periodic, this cannot be specified. The function P will depend upon the depth of V ; in the free-electron theories it was taken to be constant. Equation (9) may be regarded as the mathematical reason for the partial success of the latter.

If P were constant, the "wave number" k appearing in Eq. (9) would be related to the energy E of Eq. (8) in the manner typical of free electrons, namely,

$$E = (\hbar^2/2m)k^2. \quad (10)$$

This relation, however, is not true in general. The result (9) suggests a way for finding a more general relation. We may try to determine ψ by a process of successive approximations (a perturbation scheme), starting out with the crude form $\psi = ce^{ikx}$. In this, the "zeroth" approximation, Eq. (10) holds. The next step leads to

$$\psi = ce^{ikx} P_0(x).$$

The function P_0 is an approximation to P and is

not of interest here. At this stage of the calculation, Eq. (10) is replaced by

$$E = \frac{\hbar^2}{2m} k^2 + u(k), \quad (11)$$

and the function u happens to be *discontinuous* in k . Since this strange mathematical circumstance, which persists through all the higher approximations, is of greatest physical significance, we will describe it a little more fully.

To locate the discontinuities of u we introduce a quantity K , defined as follows. If a is the period, or spacing, of our one-dimensional lattice, then

$$Ka = 2\pi n, \quad (12)$$

where n stands for any integer. The discontinuities occur at all values of k satisfying the condition

$$k = \pm \frac{1}{2}K. \quad (13)$$

In the graph of E versus k , E takes a sudden jump at these values of k . Hence, for each k satisfying Eq. (13) there exists an *excluded* region of E -values. Permitted eigenvalues of Eq. (8) fall, as it were, into bands: the first of these extends from zero up to a value somewhat below $(\hbar^2/2m)(\pi/a)^2$; the second starts somewhat above this and ends at $(\hbar^2/2m)(2\pi/a)^2$, and so on. No electrons can possibly exist between these bands.

The free-electron picture presented a single band without discontinuities. The formation of separated bands is a change toward the case of atomic levels, which may be regarded as limiting forms of these bands taken on when the lattice spacing becomes infinite.

Thus far we have considered the case of one-dimensional motion. When the problem is treated in three dimensions, the results are very similar. The free-electron state function then has the form $ce^{i\mathbf{k}\cdot\mathbf{r}}$, where \mathbf{k} now must be regarded as a vector having components k_x, k_y, k_z . In zeroth approximation, Eq. (10) is still true. Equation (11) is replaced by

$$E = \frac{\hbar^2}{2m} k^2 + U(k_x, k_y, k_z).$$

The function U is again discontinuous; but, since it is a function of three variables, the discontinuities are now defined by *surfaces* in the space of coordinates k_x, k_y, k_z , briefly called the k -space, or momentum space. These surfaces may be pictured and drawn with the same facility and intuitive directness as surfaces in ordinary space, and a large part of our understanding of the

properties of metals rests upon detailed knowledge of these surfaces of discontinuity. The location of the surfaces depends of course upon the nature of the function V , that is, upon its periods in different space directions (lattice structure). For the simplest case, that of a cubic crystal, V has equal periods in three mutually perpendicular directions. Under that condition the discontinuities are given, not by Eq. (13) but by the relation

$$\mathbf{k} \cdot \mathbf{K} = \frac{1}{2} K^2, \quad (14)$$

of which Eq. (13) is a special form. Furthermore, the vector \mathbf{K} is now defined by $\mathbf{K} \cdot \mathbf{a} = 2\pi n$, \mathbf{a} being the lattice vector having three equal components a . This, again, is a generalization of Eq. (12). The reader will at once recognize that Eq. (14) does indeed specify a plane for any given \mathbf{K} . When the planes corresponding to all permitted values of \mathbf{K} are constructed, k -space will be divided into cells, each of which contains a continuum of energy values not connected with values in any other cell. The cells are known after their discoverer as *Brillouin zones*. Diagrams of them are given in books on solids.¹⁸ Whether or not a solid is a metal may be shown to depend on the degree to which the zones are filled with electrons.

We finally turn to a phase of the theory of metals which is perhaps the most impressive. It concerns the question: Given a collection of metal atoms in their isolated state and with their known atomic properties, how do they come to form the aggregate we call a metal? In particular, how strongly do they cohere, and what elastic forces are called into play when the aggregate is distorted?

If a calculation is to provide an answer to these questions, it must start by solving the Schrödinger equation in which the independent variables are the coordinates of all electrons contained in the assemblage, not only the quasi-free valence electrons. Moreover, this equation must be solved for a set of different distances between the atomic nuclei so as to yield the specific distance for which the eigenvalue E is a minimum. This distance will be—in the case of cubic crystals—the lattice parameter. Comparison of the energy corresponding to this minimum with the energy of the separated atoms will lead to a prediction of the cohesive energy of the metal, and the rate of change of the energy with respect to slight changes in the position of the nuclei will yield a measure of the compressibility. The task of

TABLE II. Cohesive energies of alkali metals.

Metal	Lattice parameter (Å)		Cohesive energy (kcal/mole)		Compressibility (10^{-11} cm ² /dyne)	
	observed	calculated	observed	calculated	observed	calculated
Li	3.46	3.50	39	36.2	—	0.77
Na	4.25	4.51	26	24.5	1.17	1.14
K	5.20	5.82	23	16.5	—	2.4

making such a calculation is apparent from this summary consideration. It can only proceed by judicious approximations, but it has succeeded well in the case of the simplest metals, the alkalis. Although the details are beyond the scope of this report, the results for three alkali metals, taken chiefly from the work of Seitz and Wigner, are given in Table II.

Calculations have also been made for other metals. They have led to less spectacular agreement with experience, but for reasons that can easily be traced to cruder approximations made necessary by the greater complexity of the computations involved.

4. MAGNETISM

When an atom or a molecule is placed in a magnetic field it can react in either of two ways: by taking on an additional magnetic moment, it can either *strengthen* or *weaken* the original field. In the former case it is said to be *paramagnetic*, or to have a positive magnetic susceptibility; in the latter, to be *diamagnetic*, or to possess a negative susceptibility.

To pre-quantum dynamics, that is, to all theories representing matter as a conglomerate of positive and negative charges with unquantized motions, both paramagnetism and diamagnetism were mysteries—or should have been if these theories had been pursued far enough. For it may be shown that they yield positive and negative contributions of equal magnitudes to the magnetic moments, thus canceling all magnetic effects. When Bohr introduced quantization into electron orbits this contingency was removed; when specific orbits were selected, cancelation of effects no longer took place.

The account of para- and diamagnetic effects given by recent theories, though differing from that of Bohr in cases where it modifies atomic and molecular models, does not depart from the Bohr account in fundamental conception. To explain paramagnetism it seizes upon the fact that atoms may possess permanent magnetic moments, that is, act like small bar magnets. In the absence of a

¹⁸ F. Seitz, *Modern theory of solids*, p. 288 et. seq.

magnetic field, these elementary magnets comprising a bulk substance will be oriented at random and, while each produces its own private field, the sum total of fields will be nil. In an external field \mathbf{H} , however, the elementary magnets will be oriented, so far as thermal agitation permits, with their axes along \mathbf{H} ; thus the field intensity \mathbf{H} is increased. Essentially, then, the phenomenon of paramagnetism is conditioned by the presence of permanent molecular magnets. Structures such as helium atoms, which possess no permanent magnetic moments, cannot be paramagnetic. As to the origin of these moments, the Bohr theory retained Ampère's hypothesis which supposed them to be produced by circulating charges, more specifically, electrons. Recent theory has, in addition, recognized the electron spin as an equally important contributor of permanent moments.

Diamagnetism is a universal phenomenon. Any electric current, when pervaded by magnetic lines of force, will be altered by self-induction in such manner as to cause an opposing field. That is to say, every closed electric current is a diamagnetic system regardless of how it flows. But molecules contain numerous small electronic currents and must consequently display diamagnetism. The effect under discussion is proportional to the flux enclosed by the currents and therefore proportional to the mean square of the electrons' orbital radii. Computation shows that the diamagnetic susceptibility, while omnipresent, has a value much smaller than the paramagnetic one whenever the latter is present. With atoms or molecules having permanent magnetic moments, diamagnetism is therefore masked by the stronger paramagnetic effect.

Let us now turn to some aspects in which recent theory has modified these older conceptions.

Magnetism of Free Electrons

The importance of the magnetic behavior of free electrons arises from the role they play in metals, a role discussed in SEC. 3. According to classical physics, spinless free electrons possess zero susceptibility if comprised in any finite volume. To see the reason for this, consider an assemblage of discrete charges moving without confinement. In the absence of a field, their paths will be straight lines, and these will be bent into helices when a constant field \mathbf{H} is applied. If the helical motion were undisturbed it would produce a diamagnetic reaction in conformity with Lenz's

law. Impacts with walls perpendicular to \mathbf{H} will not alter this result, but collisions occurring at boundaries parallel to \mathbf{H} will reverse the sense of the helical motion and thereby strengthen \mathbf{H} . The next impact will restore the original diamagnetic effect, and so on. On the whole there will be no magnetic contribution whatever.

Not so according to quantum mechanics. The reason is perhaps most easily understood if we replace the picture of our group of electrons moving with all conceivable speeds by their distribution in energy. The electrons then fill an energy band, the bottom of which lies at zero energy, the position of the top being a function of the temperature. The particular number of electrons predicted for each level of the band depends on our choice of distribution function (classical or Fermi-Dirac), but this does not affect our qualitative arguments. We wish to compare the properties of the energy band in the absence of the field \mathbf{H} with those possessed in its presence.

First, then, what happens to a single electron when a field is applied to it? To find the answer it is necessary to solve the Schrödinger equation with a potential energy term corresponding to the action of the field. When that is done, the energy of each electron turns out to be *increased* by the amount $(\hbar e/mc)(n + \frac{1}{2})H$, where n is an integer or zero. Hence the bottom of the band just considered is no longer at zero, but at a distance $\frac{1}{2}(\hbar e/mc)H$ above zero. Clearly, then, the field has raised the total energy of the electronic assemblage.¹⁹

But if the energy of the electrons has been increased, that of the field and hence the field intensity itself must have been reduced: the assemblage is diamagnetic. Actual values of the susceptibility depend on zone structure and temperature. The metal bismuth provides an interesting application of the preceding consideration, for it has a negative susceptibility at low temperatures which apparently cannot be explained in any other way.

Many simple metals are paramagnetic. The mechanism providing this property is to be found in the electron spin. Pauli's original explanation of it,²⁰ though later modified in detail, is still valid in principle. Adopting his picture, we again

¹⁹ The reader of course must not gain the impression that the whole band is raised bodily. A redistribution will occur which must, however, lead to an increase in total energy because both the classical and the Fermi-Dirac functions favor the lowest energies which the magnetic field has cut off.

²⁰ W. Pauli, *Zeits. f. Physik* 41, 81 (1927).

consider the energy band representing the assemblage. Each electron has a spin and hence a permanent magnetic moment which, according to the rules of quantum mechanics, extends either parallel or antiparallel to any chosen direction. Let us separate the assemblage into two groups according to the spin alinement. In the absence of an external magnetic field, the two groups are represented by energy bands which are equal in every detail, since the direction of the spin contributes nothing to the energy.

When a field is applied, however, the bands are no longer equal. The potential energy of a magnet in a field is proportional to $-\cos \theta$, where θ is the angle between magnetic moment and field intensity. Hence the electrons for which θ is 0° have their energies lowered, those for which θ is 180° have it raised. Since the displacements are independent of electron energy, one band will be lifted bodily, the other lowered. It is impossible, however, for one group of electrons to have a higher ceiling of energy than the other; collisions will equalize the tops of the bands, causing an overflow from one band to the other. As a result, more electrons have their magnetic moments parallel to the field than antiparallel to it, which is another way of saying that the assemblage is paramagnetic.

Magnetism of metals is on the whole a composition of the two opposing effects just described, plus the diamagnetism of the metallic ions. Since all three effects are relatively feeble, none of the mechanisms discussed so far is capable of explaining ferromagnetism. A proper understanding of this latter phenomenon had to await development of the finer features of quantum theory.

Ferromagnetism

So long as a solid is regarded as a collection of elementary magnets, each of which can behave independently of the others when an external field is applied, the net magnetization produced by the loosely coordinated aggregate of alinements is too weak to account for the strong ferromagnetic behavior of iron, nickel, cobalt and various alloys. Weiss recognized early that strong-arm methods—some sort of regimentation forcing large blocks of atoms into identical magnetic behavior—were needed to describe such actions. His theory assumed the presence of spin-alining forces of a nonmechanical, and indeed nonelectrical kind, forces that tie together into parallelism the electron spins of all the atoms

within a sizable region of the solid. With this hypothesis he succeeded in reproducing most of the known properties of ferromagnetic materials. The story was perfect except for one missing detail: the origin of the spin-alining forces.

Heisenberg was able to identify them with the exchange forces encountered in SEC. 2. His reasoning can be exemplified by reconsidering the analysis which informed us of the nature of a two-electron bond. Equation (4) of SEC. 2 represents, as we saw, the state function of the pair of hydrogen atoms, one at A and the other at B . We noted there that it ignores the spins of the electrons, which entered the chemical problem in a very indirect way. Here, however, the spins are all-important, and we must include them in our description.

To avoid a lengthy digression we will merely state a theorem proved in books on quantum mechanics, concerning the spin part of a state function. In the case of two electrons, this spin part, by which the "orbital" part [Eq. (4) of SEC. 2] must be multiplied, is *symmetric* with respect to electron exchange if the two spins are parallel, *antisymmetric* if they are antiparallel. Let us denote the symmetric spin function by X_+ , the antisymmetric, by X_- . In its fullness, the state function therefore reads, either

$$\varphi_{\text{parallel}} = [a_1 u_A(1) u_B(2) + a_2 u_B(1) u_A(2)] X_+$$

or

$$\varphi_{\text{antiparallel}} = [a_1 u_A(1) u_B(2) - a_2 u_B(1) u_A(2)] X_-.$$

But each φ must satisfy the exclusion principle, which requires that $a_2 = -a_1$ for φ_{par} and $a_2 = +a_1$ for φ_{anti} . Thus we must write

$$\begin{aligned} \varphi_{\text{par}} &= a [u_A(1) u_B(2) - u_B(1) u_A(2)] X_+, \\ \varphi_{\text{anti}} &= a [u_A(1) u_B(2) + u_B(1) u_A(2)] X_- \end{aligned} \quad (1)$$

Using each of these functions in turn, we compute the energy of the pair of atoms:

$$E = \int \varphi^* H \varphi d\tau.$$

Then we conclude that, of the two functions φ_{par} and φ_{anti} , the one that gives the *smaller* E will be realized in nature. If it happens to be φ_{par} , we have an explanation for the alining tendency.

The spin functions themselves are normalized and contribute the factor 1 to E . The expression for φ_{anti} is, of course, the same as Eq. (5), SEC. 2; that for φ_{par} differs in some signs. Thus,

$$E_{\text{par}} = 2E_H + \frac{e^2}{R} + \frac{2J + J' - 2K\Delta - K'}{1 - \Delta^2},$$

$$E_{\text{anti}} = 2E_H + \frac{e^2}{R} + \frac{2J + J' + 2K\Delta + K'}{1 + \Delta^2}.$$

Now Δ always lies between 0 and 1 so that the denominator of the last term is always positive. In fact, Δ^2 often can be neglected. Then E_{par} is smaller than E_{anti} , and the parallel arrangement of spins is stable, if the exchange integrals K and K' are positive.

Our simplified discussion has dealt with hydrogen atoms. The situation in a metal differs in many quantitative respects since neighboring ions have more than one electron, and the electrons are partly free. But the essential result remains: the magnetic moments of neighbors will be parallel if the exchange integrals between them, evaluated for their actual distance of separation in the crystal, are positive. This, then, is the criterion for ferromagnetism. Weiss' mysterious forces have turned out to be the exchange forces of quantum mechanics.

The picture given here is too simple for quantitative application because the exchange integrals with respect to electrons in a metal are much more complicated than those considered. It does, however, furnish a basis for understanding why ferromagnetism should occur in the iron group: here atoms have incomplete d - and f -shells of electrons whose wave functions favor positive exchange integrals, as closer inspection shows.

It is hardly justifiable to conclude this section without pointing to the fruitful interplay between the researches in the fields of magnetism and of spectroscopy. The latter discloses the detailed structure of atoms and molecules. From this knowledge of the structure, theory deduces the magnetic properties; and magnetic experiments invariably bear out the deductions. The Zeeman effect, of which modern quantum mechanics renders beautiful account, is particularly notable as an example of this interrelation. Limitation of space forbids our inclusion of this topic.

5. NUCLEAR PHYSICS

Although the nucleus played an important role in atomic structure before 1925, it was not until the advent of quantum mechanics that investigations regarding nuclear structure advanced. But any hope that nuclear structure would be rendered simple and understandable by the new

theory was soon quashed. Difficulties of rather large stature arose. What law of force, for instance, accounted for the cohesion of nuclear particles within nuclei? Moreover, the concept of a nucleus composed of protons and electrons did not agree with many experimental facts.

Composition of the Nucleus

With an abundance of data on nuclear transformations and with the discovery of the neutron in 1932, theoretical nuclear physics began to move faster. A nucleus composed not of protons and electrons but of protons and neutrons was adopted. Let us review briefly the reasons for this change, since they were of importance in all nuclear theory that followed. By measurement of the intensities of alternate lines in the band spectra of homonuclear molecules (alternating intensity phenomenon), it is possible to determine the spins of many nuclei. These alternations of intensity are due to the different degeneracies associated with two nuclei having parallel or antiparallel spins (see end of SEC. 1). All these spin determinations showed that nuclei of odd atomic weight have half integral spins, whereas those of even atomic weight have integral spins. Spin determinations from the hyperfine structure of spectral lines and from the Stern-Gerlach effect confirm these conclusions. Thus the proton is found to have a spin of $\frac{1}{2}$ in units of \hbar . Since the electron is known to have a spin of $\frac{1}{2}$, the resultant spin of a deuteron made up of two protons and an electron would be either $\frac{3}{2}$ or $\frac{1}{2}$. Such is not the case. Band spectra measurements show that the deuteron has a spin of 1. Similarly, the nucleus N^{14} also has a spin of 1, but if it were made up of protons and electrons the spin would have a half-integral value. Thus the assumption that electrons are present in the nucleus does not lead to consistent results so far as the nuclear spin is concerned. But if we assign a spin of $\frac{1}{2}$ to the neutron²¹ we are no longer faced with this inconsistency. For now a deuteron is composed of two elementary particles (proton and neutron) each with a spin of $\frac{1}{2}$. Thus we would expect a resultant spin of 0 or 1. Likewise N^{14} with 14 elementary particles should have an integral spin.

Another generalization arising from data on the alternating intensities in rotational band spectra is that nuclei of even atomic weight follow Bose-Einstein statistics, while those with

²¹ Assigning a spin of $\frac{3}{2}$ to the neutron would also yield correct results. But simplicity together with the fact that other elementary particles have spins of $\frac{1}{2}$ argues strongly for the lower value.

odd atomic weight follow Fermi-Dirac statistics. Bose statistics require that the state function representing several similar systems or groups of particles be symmetrical; the function does not change sign upon interchange of two systems. Fermi statistics deal with antisymmetric wave functions. The Pauli principle requires that the state functions representing several similar elementary particles be antisymmetric with respect to particle interchange. The interchange of one particle with another changes the sign of the wave function, while a second interchange brings back the original sign. Hence the interchange of pairs of particles leaves the wave function unchanged. In general, the interchange of an odd number of particles changes the sign of the wave function, whereas interchange of even numbers of particles does not affect it. On the basis of this result, a deuteron composed of two protons and an electron would show antisymmetrical behavior as far as interchange of the nuclei is concerned, thus following Fermi statistics. The deuteron obeys Bose statistics; hence we have a contradiction with experiment. An analogous situation arises with regard to N^{14} . These difficulties are removed if the nucleus is composed of neutrons and protons. For interchange of two deuterons leads to the interchange of a pair of particles, leaving the wave function unchanged. Incidentally, it should be noted that the neutron itself must have an antisymmetric wave function. This constitutes an *a posteriori* proof of the Pauli principle.

A third telling argument against the presence of electrons in the nucleus is the impossibility of having a potential barrier that is high enough to keep the electrons inside. A solution of the relativistic Schrödinger equation, with a potential barrier sufficient to account for observed half lives of electron-emitting nuclei, would require a potential that would overlap the *K*-shell of electrons. Thus the potential acting on these electrons, and even on the more distant ones, would be altered beyond recognition. These large perturbations are not observed, thus disproving the presence of a potential barrier of the type required.

Agreement between experiment and theory thus demands a nucleus made up of neutrons and protons. But if electrons do not exist in the nucleus, how can the existence of beta-particle-emitting nuclei be accounted for? This question can be answered by asking another. How can the existence of light-quantum-emitting atoms be

accounted for if the atom is not composed of quanta? The generally accepted answer is that a light quantum is born in the moment of its emission. The same point of view should be taken with respect to the beta-particle; it is produced in the moment of its ejection from the nucleus.

Having decided upon the particles making up the nucleus, we are faced with the problem of intra-nuclear forces.

Nuclear Forces

The application of quantum mechanical principles to atomic structure has been an unqualified success. Thus there is a strong supposition that questions concerning the nucleus will also yield to quantum mechanical attack, especially since approximate calculations show fair agreement with experiment. Since nuclear structure depends upon the forces between nuclear particles, information concerning their nature is important. The principal attractive forces existing in the nucleus cannot be electric in nature, since the neutron possesses no charge. We do know that a Coulomb repulsion exists between protons; but since the nucleus is stable, this force must be small compared to those holding the nucleus together.

The first bit of experimental evidence which we can make use of is the approximate relation between atomic weight and number for the lighter stable elements up to about calcium. An inspection of the data for these elements shows that the atomic weight is twice the atomic number. Since the atomic number represents the number of protons in the nucleus, the number of neutrons must equal the number of protons. We may interpret this as showing that the largest attractive forces are those between neutrons and protons. If this were not so, light nuclei would show a trend towards a preponderance of either of the two particles. Moreover, we can reason that neutron-neutron and proton-proton forces in the nucleus are about the same, if we disregard the Coulomb repulsion between protons. If, for example, the force between two protons were larger than that between two neutrons, the condition of maximum stability of the nucleus would be shifted towards a relatively larger number of protons. When the Coulomb repulsion between protons becomes appreciable, as it does for heavy nuclei, then we would expect maximum stability for a nucleus with more neutrons than protons. Such is the case, for heavy nuclei have atomic weights considerably larger than twice the atomic number.

What type of force is it that can account for the attraction between a proton and a neutron? The answer may be found by obtaining a measure of the stability of the various nuclei. If the atomic weight of a nucleus is computed by adding up the weights of the individual protons and neutrons, a value larger than the actual atomic weight is obtained. Owing to the equivalence of mass and energy, the energy of the nucleus is less than the sum of the energies that its component particles have in the free state, a requisite condition for stability. By use of Einstein's relation, $energy = mc^2$, the difference in mass, known as the mass defect, may be converted to energy units. Thus the mass defect gives us direct information concerning the binding energy of the nucleus. It is found experimentally that the mass defect, and hence the binding energy, increases linearly with atomic weight. This fact is of great importance, for it means that the binding energy of a nucleus is proportional to the number of particles in the nucleus. If each particle in the nucleus interacted with every other particle, the interaction energy, and hence the binding energy, would roughly be proportional to the square of the number of particles. Since this is not the case, we can rule out forces of this nature as playing an appreciable role in the nucleus. Thus, in effect, we have said that nuclear forces show the property of saturation; each particle does not interact with all the others, but only with a preferred few. Forces of this type are well known in quantum mechanics. They are the exchange forces treated in SEC. 2. In the case of molecules the exchange forces may be interpreted as due to the oscillation of one or more electrons among the atoms. The success of these forces in explaining the homopolar or covalent bond is ample reason to carry the mathematical formalism associated with them over into the domain of nuclear physics.

The simplest nucleus to consider is the deuteron. It was first suggested by Heisenberg that exchange forces be used which depended upon the relative spins of the two particles. If this were correct, one may show that the deuteron should be a saturated structure. Now the deuteron has a binding energy²² of 2 Mev while that of the alpha-particle is 28 Mev. Thus the binding energy per particle in the alpha-particle is about seven times that in the deuteron. Therefore the alpha-particle must be regarded as showing more saturation than the deuteron. The conclusion we arrive at, then, is that the forces between nuclear

particles do not depend largely upon spin orientation. Moreover, in accordance with the Pauli principle, two neutrons and two protons can be in the same quantum state owing to two possible spin orientations. Thus a second conclusion from the large binding energy of the alpha-particle is that protons and neutrons exert strong forces on each other when they are in the same state. On this basis, the force between a neutron or proton and the saturated alpha-particle would be small, which yields at once an explanation for the nonexistence of a stable nucleus of atomic weight 5.

Because the deuteron is not a saturated structure, Majorana suggested that the exchange forces between nuclear particles are independent of spin orientation. But before the problem of the deuteron can be solved, it is necessary to choose a potential function relating the interaction energy to the inter-particle distance. The actual form of this function is not critical so long as it represents a deep and narrow potential "well." A solution of the Schrödinger equation together with the knowledge of the binding energy of the deuteron gives about 4×10^{-13} cm as its radius, and about 2×10^{-13} cm as the range of the forces. Moreover, it also turns out that the ground state is the only stable state if we disregard the spins. If we take the spins into account, we would expect a second level which differs from the ground states in that the spins of neutron and proton are antiparallel. If we assume pure Majorana forces, the difference in energy between those two levels is about 100 ev, while experimentally the difference is in the neighborhood of 2 Mev. This can be explained only by assuming forces that are slightly spin-dependent. To summarize, the forces between nuclear particles are of short range and of the exchange type. They operate mainly between particles in the same quantum state and show a dependence upon the spin orientation of the interacting particles.

Data on the scattering of nuclear particles have been of great value in deciding upon the determining factors related to nuclear forces. The scattering of protons by protons has permitted the calculation of the width and depth of the potential "well" mentioned previously. These results show that proton-proton forces are, if anything, slightly smaller than proton-neutron forces. The scattering of neutrons by protons has been the chief evidence for the existence of spin-dependent forces. The most clear-cut experiment was the scattering of neutrons by ortho- and

²² 1 Mev = 1 million electron volts.

para-hydrogen. Ortho-hydrogen consists of molecules whose atoms have parallel spins; in para-hydrogen, the atoms have antiparallel spins. Owing to the presence of spin-dependent forces, slow neutrons can cause a transition from ortho- to para-hydrogen, the latter being the ground state. In this case the neutron scattering is very large and ortho-hydrogen is said to have a large cross section. The scattering cross section for the ortho variety is about eight times that of the para; this may be regarded as ample proof of the existence of spin-dependent forces.

So far we have not mentioned the mechanism by means of which the exchange forces arise. While this mechanism does not enter into the formalism of setting up a nuclear wave equation, a knowledge of it is of great interest. It would be most satisfying if the exchange force were due to the interchange of an electron and a neutrino²³ between the attracting particles. Unfortunately the force produced by this mechanism is too small. To remedy the difficulty, Yukawa suggested that the interchange of a particle with an electronic charge and a mass about 150 times the electron mass would fit the facts (see paper II, SEC. 4). The discovery in 1936 of a particle of this type in cosmic rays lent weight to this theory. Further verification of it must probably wait until mesons, as these particles are called, are produced in a laboratory, a feat of which the largest cyclotrons may be capable.

Nuclear Structure

We know that the nucleus is composed of neutrons and protons. But how are these particles arranged? Do they form themselves into alpha-particles? This point of view seems fruitful, especially since alpha-particles are emitted from radioactive nuclei. If the building blocks of the nucleus are alpha-particles, then a large exchange force must be present to overcome the electrostatic repulsion between them. Such a state of affairs might be accounted for by a constant exchange of neutrons and protons between the various alpha-particles. Wheeler²⁴ has applied the theory of groups to this and other arrangements of particles, and his method shows promise of fruitfulness.

²³ If energy, spin and statistics are to be conserved in beta-decay, it is necessary to assume that the emission of an electron is accompanied by that of a neutrino, the neutrino having small if not zero mass, spin of $\frac{1}{2}$ and Fermi statistics. Positive evidence for the existence of the neutrino has not yet been produced.

²⁴ J. A. Wheeler, Phys. Rev. 52, 1083 (1937); see also E. Wigner, Phys. Rev. 51, 1061 (1937).

The Schrödinger equation has been applied to several simple problems of the nucleus with fair success. One of these is the emission of alpha-particles. Classically, such a process is very difficult to explain. Either the alpha-particle gets out at once or it stays inside the nucleus forever; there is no "maybe" associated with this situation. Quantum mechanically, however, the state of affairs appears quite different. The alpha-particle inside the nucleus is separated from the outside by a potential "hill." To get outside, the particle either may obtain enough energy to go over the top or it may "tunnel" through. It is this tunnel effect that is foreign to classical physics. It arises from the fact that an alpha-particle inside with an energy smaller than the top of the barrier has a finite probability of being on the other side of the hill with the same total energy. A complete analysis of the problem shows that in order to derive the experimental Geiger-Nuttall law relating half life to range it is necessary to choose a radius for the nucleus of about 8×10^{-13} cm.

The success of the energy level concept in atomic physics has led to attempts to apply it to nuclear physics. Evidence for energy levels comes from alpha-, beta- and gamma-ray spectra, which permit experimental determination of energy level structure. The agreement with theory is fair. As an example let us consider an alpha-particle in an infinitely deep potential hole; that is, $V=0$ for $r < r_0$ and $V = \infty$ for $r > r_0$. Inside the nucleus the Schrödinger equation takes the form

$$\nabla^2 \psi + \frac{2m}{\hbar^2} E \psi = 0.$$

If we separate variables by setting

$$\psi = R(r) \Theta(\theta) \Phi(\varphi),$$

we obtain for the radial part,

$$R'' + \left(\frac{2m}{\hbar^2} E - \frac{l(l+1)}{r^2} \right) R = 0,$$

where l is the orbital quantum number. For the special case in which l is zero, the solution of the foregoing equation has the form

$$R = c \sin kr,$$

where $k^2 = 2mE/\hbar^2$. Since the particle cannot get out, R must vanish at r_0 . This will be true when $k=0, \pi/r_0, 2\pi/r_0, \dots$, or for energies $E=0, \pi^2 \hbar^2 / 2mr_0^2, 4\pi^2 \hbar^2 / 2mr_0^2, \dots$. Thus the energy levels go up as the squares of the integers in this

case. For $l \neq 0$ the solutions take the form of Bessel functions of index $(l + \frac{1}{2})$ with argument kr . To find energy levels for this case the zeros of the Bessel functions must be found from tables. If a finite hole had been chosen, the general scheme would be the same but the levels would be depressed. To obtain agreement with experiment a value must be chosen for r_0 . In general this value will differ somewhat from the one in the radioactive decay problem, but is of the same order of magnitude. The values of r_0 found by different methods differ by as much as 50 percent. This is a situation analogous to the determination of molecular radii, and divergence among the results is to be expected.

This simple picture, which assigns energy levels to individual particles within the nucleus, is of course a very crude approximation. More adequately, the whole nucleus must be treated as a many-particle system with energy levels referring to the motion of all constituents. Such a system of levels is very complex and extremely difficult to calculate, even when a simple nuclear law of force is chosen.

6. EXPERIMENTAL MIRACLES

The preceding material has largely been a development of various branches of quantum mechanics, and an exposition of applications and results of this theory. To the physicist the achievements of the theory are both astounding and gratifying. Equally important, however, are the experimental advances which have been made at a time when theory threatened to go far ahead of experiment. We refer to the development of the cyclotron, the betatron and the electron microscope. Twenty years ago these devices would have been considered as miracles. Today they are more familiar, but even after constant association with them, one still contemplates them with awe. We feel that a brief description of the devices must be included in an article of this sort, even if its chief preoccupation is meant to be theoretical.

The Cyclotron

In an effort to gain information concerning the properties of the nucleus, physicists had embarked on a program of taking the nucleus apart in order to probe its secrets. Certainly the simplest way to do this was to break it into parts by striking it with a projectile. For this purpose energetic high-speed particles of nuclear size were needed. The cyclotron was an instrument

designed to meet this need. By the use of relatively small potential differences, it produces particles with energies well over 10^6 ev. Alpha-particles of 34 Mev and deuterons of 17 Mev have been produced at currents of about 300 μ amp.

The basic theory of the cyclotron is so simple that it is rather surprising, in retrospect, that the first one was designed and constructed as late as 1932. It is well known that a charged particle²⁵ of mass m and charge e moving in a plane perpendicular to a magnetic field of uniform intensity H moves in a circle with angular velocity

$$\omega = \frac{e}{m}H. \quad (1)$$

The importance of this relation lies in the fact that the angular velocity, and hence the period of rotation, is independent of the linear velocity of the particle and of the radius of its orbit. The cyclotron takes advantage of this fact. It consists of two hollow electrodes each shaped in the form of a **D**. The two dees, a small distance apart, are placed in a vacuum chamber with their straight sides parallel. The electrode structure is situated between the poles of an electromagnet which gives rise to a uniform magnetic field perpendicular to the plane of the dees. When the cyclotron is operating, an alternating potential difference V of frequency $\omega/2\pi$ is applied to the dees, giving rise to an alternating field between them. If at the moment when the electric field intensity is a maximum, an ion of charge e and mass m is produced at the center of the space between the dees, it will be accelerated into the interior of one of them. Here the electric field intensity is zero, but owing to the magnetic field the ion will transverse a circular path and arrive at the space between the dees when the electric field is a maximum in the other direction. Again it will be accelerated, this time into the interior of the second dee, where it travels along a circular path of larger radius owing to its increased velocity. Thus for each half revolution the ion has its energy augmented by falling through the potential difference V , and travels in semicircles of ever increasing radius until it passes out through an exit window. If the ion has made 50 rev, receiving an increment of 50,000 v twice in each cycle, then upon emerging its energy would be 5 Mev. Ions that are not subject to the maximum potential difference when they cross the gap between the dees will also emerge with this energy, but these ions must travel longer dis-

²⁵ Cgs electromagnetic units will be used throughout.

tances and hence there is less chance that they will get through without colliding with residual gas atoms on the way.

By use of Eq. (1) the kinetic energy of the ion when it emerges is found to be

$$E = \frac{1}{2} \frac{H^2 e^2}{m} r^2, \quad (2)$$

where r is the radius of the final semicircular orbit. Thus the energy attainable with the cyclotron is intimately connected with the magnetic field and the radius of the dees. Practical difficulties place the maximum magnetic field in the neighborhood of 20,000 oersteds, thus requiring an increase in r if larger values of the energy are wanted. Hence the new cyclotron being constructed at Berkeley, California, will have a dee radius of about 75 cm. Several major difficulties arise in the construction of a cyclotron of these dimensions. First, the maximum potential difference between the dees must be of the order of 10^6 v if the ions are to travel a short enough distance so that their chances of colliding with gas atoms are small. To achieve a potential difference of this magnitude at frequencies of about 10 megacycle/sec is no small undertaking. Second, at very high energies the change in mass with velocity becomes appreciable. The simple relation (1) then no longer holds, and the ion's motion is no longer in "resonance" with the alternating potential difference. This may be counteracted by a suitable increase of the magnetic field intensity in the high-energy region of the ion path. However, on removing this difficulty another obstacle is encountered. For it may be shown that a radially increasing field has a defocusing effect upon the beam of ions, causing them to strike the walls of the dees. Thus the construction of a large cyclotron involves many problems which may be ignored in the case of a small one. The problems are by no means insoluble, and when the new cyclotron is finished it should produce ions of about 100 Mev.

The wealth of data accumulated by use of the cyclotron may be judged by the fact that at present about 400 artificially radioactive nuclei have been found, whereas, in 1932 none was known. So important has the cyclotron become that most laboratories doing work on nuclear problems possess one.

There is a limitation inherent in the cyclotron—it cannot be used to accelerate electrons. For even 500-v electrons have a velocity comparable to that of light, and the mass correction is so large

that Eq. (1) is entirely inapplicable. Moreover, the frequency of the alternating potential would have to be outside the limit attainable at present, owing to the small mass of the electron. A device that can be used to give electrons high kinetic energies was constructed in 1941 by Kerst.²⁶ It goes under the names of "electron induction accelerator," or *betatron*.

The Betatron

The betatron takes advantage of the change in the mass of an electron with velocity. The fundamental principle behind the device is that of the production of an electric field by a changing magnetic flux. The betatron is so designed that the electrons travel in an orbit of constant radius r . If the total flux through this circuit is ϕ , the magnitude of the emf around the circuit is $\dot{\phi}$, the time derivative of the flux. The electric field strength will thus be

$$E = \dot{\phi}/2\pi r. \quad (3)$$

Applying Newton's second law, we get

$$\frac{d}{dt}(mv) = \frac{e\dot{\phi}}{2\pi r}, \quad (4)$$

or

$$\Delta(mv) = e\Delta\phi/2\pi r.$$

Equation (4) does not tell the entire story, since an additional requirement is that the magnetic field at the orbit be sufficient to keep the electron in its circular path. Thus, from Eq. (1),

$$mv = erH, \quad (5)$$

or

$$\Delta(mv) = er\Delta H.$$

Combining Eqs. (4) and (5) we obtain the necessary relation between H and ϕ :

$$\Delta H = \Delta\phi/2\pi r^2. \quad (6)$$

The reader will note that $\Delta H \cdot \pi r^2$ is the change in flux if the field is uniform throughout the orbit. Equation (6) tells us, on the other hand, that this quantity is just half of the total change in flux. Hence the conclusion is that the field strength at the circular path must be weaker than in the interior. The weak field holds the electron in its path, and the strong field supplies the accelerating force. This condition may be fulfilled in practice. Luckily enough this radially decreasing field also satisfies the necessary focusing requirements.

The glass container in which the electrons make their circuits has the shape of a doughnut. This vacuum chamber is placed between an electromagnet of such a shape that it fulfills the require-

²⁶ D. W. Kerst, Am. J. Phys. 12, 219 (1942).

ments of Eq. (6) and also the focusing conditions. In the latest accelerator the electromagnet is energized by a 180-cycle/sec generator. That part of the alternating field which builds up from zero to a maximum is used to accelerate the electron. In the time corresponding to this quarter of a cycle, an electron will go about 10^8 cm, a rather appreciable distance. One might suspect that a very high vacuum is necessary if the electrons are to get through. In actual practice a pressure of 10^{-6} cm-of-mercury is low enough for operation. When the field reaches its peak, auxiliary coils are energized which push the electron out of its orbit and so permit it to strike a target.

The most recent betatron constructed produces 20-Mev electrons at an average current as high as 1 μ amp. The x-rays resulting from these electrons have energies comparable to those of the most energetic gamma-ray. Considering the youth of the betatron this is a result to marvel at.

The Electron Microscope

The third spectacular advance of the last two decades was the development of the electron microscope. The very fact that such an instrument exists is a tribute to the soundness of quantum mechanics. It not only represented a triumph to the theory but also opened up an entirely new field of experience to the scientific investigator.

It is well known that a theoretical limit exists for the resolving power of an optical instrument. The smallest object that may be perceived through a microscope is limited, owing to diffraction effects, by the wavelength of the light employed. Thus while light may be considered as traveling in straight lines in many cases, the wavelike properties of light must be considered in attempting to detect very small objects. The possibility that particles should also exhibit wavelike properties was first suggested by de Broglie (see paper I). He proposed to associate with a particle of momentum p a wavelength²⁷

$$\lambda = h/p. \quad (7)$$

A few years after this supposition was made, Davisson and Germer, and independently Thomson, published the revolutionary results of experimental investigations on electron diffraction. They showed that electrons acted as waves, with a wavelength given by Eq. (7). According to this

expression, an electron with an energy of 150 ev has a wavelength of 1A associated with it. Electrons with higher energies have still smaller wavelengths. Hence the electrons could be considered as traveling in straight lines with diffraction effects far in the background.

Since 1900, microscopy had been limited by a lower wavelength of 3000A. Now its shackles were off and development of suitable magnetic and electrostatic focusing systems led to the appearance of the first electron microscope in the early 1930's. Since that time many improvements have been made in the instrument. For example, electron energies have increased from 300 to about 10^6 ev, thus permitting greater penetration of the electrons and hence the use of thicker samples for observation.

Objects of size about 10^{-6} cm may be "seen" by the electron microscope, while ordinary light can only be used down to 2×10^{-4} cm. Thus the electron microscope has reached the fringe of molecular dimensions. The larger organic molecules, such as the tobacco mosaic virus, can be photographed. Other viruses invisible with ordinary light can now be observed. The list of applications may be extended almost indefinitely, and the demand for the instruments is obviously great since they are available commercially. Yet the electron microscope is not a finished product—improvements are continually being made.

CONCLUSION

It would be inappropriate to close this review without some mention of the theoretical problems that lie ahead. Many of these problems have been alluded to in previous sections. The theory of metals and solids is by no means in finished form, although the "band approximation" is an excellent beginning. The liquid phase, lying between the completely disordered gaseous and the ordered solid phase, still presents vast unexplored regions. Likewise, our understanding of nuclear structure is by no means complete.

Solution of these and similar problems seems to involve an almost forbidding amount of cumbersome calculation. A discouraging situation of this sort has always arisen in the backwash of striking advances in physics. Despite their tedium, the tasks are important because they pave the way for further progress by pointing to specific defects of present knowledge. And it may be hoped that the development of machines to perform the calculations will make the work less formidable.

²⁷ See paper II, Sec. 4. The solution of Dirac's equation for the free electron gives $\psi = A \exp(ipx/\hbar)$. Thus here also, $\lambda = h/p$.

Mathematics for the Student of Elementary Physics

R. B. LINDSAY

Brown University, Providence, Rhode Island

AT the Southport meeting of the British Association in 1903, Sir Arthur Schuster, then professor of physics at the University of Manchester, put forward a remarkable example of the power of mathematics in dealing with chemical substances. He began innocently enough with the well-known chemical equation,



and proceeded to submit it to the simple transformation



which is obviously of the form

$$(\text{H} - \text{Cl})^2 = 0;$$

whence he drew the inevitable conclusion that

$$\text{H} = \text{Cl}.$$

The identity of hydrogen and chlorine thus proved by the most rigorous algebra was expected to lead to great simplification of the science of chemistry. Schuster went on to draw other equally interesting mathematical conclusions, though how he ever managed to get through his quarter-hour of attempted leg-pulling at the expense of staid British scientists, I have no idea.

Marvelous is the power of mathematics in the hands of both the skilled and the unskilled: we all know the interesting results our elementary physics students often produce with it and the sublime confidence they repose in those figures which are guaranteed by the Deity never to lie!

Now it might be thought from these introductory remarks that I am getting ready to say that it would be better for the student of elementary physics in the liberal arts college to have no mathematics at all at his disposal. I hasten to assure you that this is by no means the case. No education can pretend to be liberal unless it gives the student something worth while to think about and talk about, and it certainly cannot pretend to have achieved its end in any real sense unless the student knows what he is talking about. The value of mathematics in the study of physics for the liberal arts student must be judged, in my opinion, solely on this basis: does it assist the student to more accurate and

profitable thinking about physics? If it does not, it is worse than useless. And this is certainly the case if it reduces to mere manipulation of meaningless symbols.

Physics is a description of a certain portion of human experience carried through by means of a methodology which physicists, at any rate, think has proved successful. The attempt to acquaint the student with physics as a part of a liberal education must make clear to him what this method is. But this involves the necessity for logical thinking, or more humbly stated, the putting of two and two together. Now logical thinking is essentially mathematical in character. The seeker after light in physics must expect at the very least to use this part of his mathematical background; otherwise there will be no light but the darkness of a confused conglomeration of miscellaneous facts, which unfortunately some times passes for knowledge. It is reported that Clerk Maxwell once remarked that everyone is a mathematician, but that some know it whereas others don't. In our physics classes we are very apt to get those in the second category. We should consider it our task to provide enlightenment, and if properly handled the process can be a more exhilarating experience than that of our colleagues in elementary college mathematics, who evidently still feel themselves under the necessity of stressing drill in manipulation.

It seems to me we have the right to expect that the student who embarks on a course in college physics as a part of an education in liberal arts should come to us with the usually required school arithmetic, algebra and geometry. From the point of view of an appreciation of the method of physics, geometry is of particular importance. If taught properly in the secondary school, geometry is really in its essence a physical science. It abounds with experimental laws governing the properties of figures drawn on paper, which the student can readily verify for himself with the use of ruler, protractor and planimeter. At the same time it possesses the logical structure characteristic of every physical theory with its primitive, undefined notions, its conceptual definitions or constructs, its postulates and its theorems. The theorems are of course the logically deduced laws of geometry. It is probably too

much to hope that all secondary school teachers can be prevailed on to teach geometry from this point of view. However, no matter how it is taught, the results are probably more effective than those secured by high school physics.

Besides its methodological significance, another obvious value of high school geometry in the study of college physics is the encouragement it gives the student to construct accurate diagrams in the endeavor to visualize physical situations correctly. No matter on what mathematical level college physics is taught, even the very lowest, if the student is not made to cultivate the habit of constructing simple diagrams to guide him in his thinking he is pretty sure to emerge with some decidedly queer, not to say wishy-washy ideas. Perhaps to avoid misunderstanding I ought to make it clear at this point that my conception of physics for liberal arts students does not imply merely a collection of pretty lecture demonstrations accompanied by the remarks: "Now, isn't that interesting!", or "Isn't this a remarkable result." We must have the honesty to realize that no student learns physics merely by having his attention called to it. In fact, I believe that nothing is learned in this way. Learning physics is a process which consists of firmly attaching the seat of the pants to the seat of the chair and thinking about the lecture material or the book material, preferably with the use of a pencil and paper with which to explain to oneself what one actually does or does not understand. It seems to be psychologically well attested that the mental activity involved in this process is much assisted and stimulated by the physical activity of the symbolical representation of concrete situations, in plain language, by drawing pictures. The student who has learned geometry has already attained to some appreciation of the value of this process. Without geometry he is apt to think of the matter as one involving artistic considerations alone and may well wind up with a picture of his professor, usually not too flattering.

Many of you will now be inclined to exclaim: "This may be all very well, but aren't the wrong things being stressed? Isn't the one important mathematical thing about physics which we must strive to get across to the student the fact that physics is a *quantitative* science, involving measurements and therefore numbers? Isn't arithmetic the branch we should stress?" I hasten to make clear that I understand this point of view and have not forgotten it. However, in the case of the student exposed to a single course in

physics for cultural purposes and with no thought of using the subject as a professional tool, this aspect is secondary to the one I have just been emphasizing. When time is limited it is well to concentrate on the important thing first, and for the liberal arts major that is the fundamental method of physics. Of course illustrative examples are necessary, and for these numerical work will be essential. A few comments on arithmetic are in order here.

My own experience has been that of all the mathematics which the student is called upon to use in elementary physics he finds arithmetic the biggest bugbear. Just where the difficulty lies I have never been quite able to make out, but I suspect it is to be found, first, in a lack of practice on practical problems, and second, in the omission of emphasis in grade and high school mathematics on simple, rapid methods of estimating the order of magnitude of results of numerical calculations instead of doing what a lot of students manage to do, namely, cover the whole page with figures and finally wind up with the wrong decimal point. After all, in illustrative problems it is the order of magnitude that counts and not the answer to five or six figures of which only two or three may be significant. Possibly I am asking for too much, but I wish it might be possible to teach students to use what may be called the power-of-ten method. If he could be made to express all numbers in terms of a simple number with one digit to the left of the decimal point multiplied by an appropriate positive or negative power of ten, he could then readily estimate the order of magnitude of the result of any calculation with a minimum of manipulation, provided only he has at his disposal the multiplication table up to 9 times 9. This scheme of course fits perfectly with the use of the slide rule, though it does not necessitate it. More important still, it reinforces the desirable emphasis on significant figures in physical problems.

If I have said little about the use of algebra, it is because I am rather sceptical of its elaborate use in the type of physics course we are supposed to be discussing. We all know how many students regard physics as merely a game in which the solution of every problem is the finding of the "right" formula, that is, the one which has the right number of symbols to meet the numerical data provided. As a game this may conceivably encourage quickness of wit, but I have never been able to see that it has any connection with physics or that it encourages the student to do

any thinking about what physics really means. We are likely to forget that though it is perfectly appropriate for the person who understands physics to use analysis as the natural language in his everyday work with the science, it is foolish to encourage the student to use this analysis until he has a clear conception of its meaning, and this of course he can never get merely from the formulas themselves. While the student is learning a particular subject, each illustrative problem should be solved by a stepwise process of reasoning in which each step involves the simplest possible mathematical analysis and in which the physical common sense is never lost sight of; for then there will be a minimum of mystery.

I think we have all been impressed by a fundamental weakness in the algebra instruction in the secondary school, as far as concerns its possible application to elementary college physics. The algebraic symbols which the student learns to juggle have no meaning except as counters in a game in which the rules merely relate to the permissible operations on the counters. We do not often appreciate the tremendous shock experienced by the student when he encounters algebraic manipulation with symbols which have definite physical meaning because they represent physical operations in the laboratory or elsewhere. Consequently, if algebraic symbolism is used in teaching elementary physics, great pains must be taken to emphasize the physical meaning of every relation brought to the attention of the student. He should never be satisfied with an equation unless it conjures up for him a clear picture of a class of concrete situations. But perhaps I had better not explore this point too far, lest I run the risk of winding up my remarks by frothing at the mouth over the basic weakness of our methods of teaching theoretical physics

instead of sticking to my subject of cultural elementary physics.

I cannot close without putting in a word in favor of stressing to the liberal arts student the essential beauty of mathematics as the most wonderful language ever created by man, the one true universal language. At the same time it will do no harm to emphasize that this language was made originally not merely to play with but to say something with about the world in which we live. Perhaps a little of that point of view might actually percolate back to our pure mathematical colleagues and assist in some small measure in meeting the situation rather pointedly implied in the late Sir J. J. Thomson's story of one of his students who attended some mathematical lectures in order to learn how to use Taylor's theorem but finally gave up because the lecturer had talked about nothing but cases where the theorem could *not* be used. What is needed is a little more collaboration between the departments of mathematics and of physics in order to emphasize that we are not in the business of trying to do wholly different things—that it would be mutually helpful if the mathematicians did not consider it beneath their dignity to teach a little physics while we should certainly lose no opportunity to encourage the student to use his mathematics and if possible strengthen his grasp of it. Such collaboration might well be part of a general movement toward breaking down departmental barriers of all kinds. The liberal or, more precisely, the liberating character of physics is proportional to the impact it makes on all fields of human activity; and the success of the teacher of the liberal arts student will largely depend on the success with which he makes the student realize the magnitude and intensity of that impact. Here is a challenge that the resourceful teacher will not disdain to meet.

J. J. always illustrated his general result by a numerical example, realizing that if this is not done, the general result will to many be a mere piece of technical jargon, which the class may be able to reproduce, but which they will not understand. He had the invaluable habit of saying everything over again, in different words. The listeners who find this superfluous are few indeed.—LORD RAYLEIGH, *The Life of Sir J. J. Thomson* (1942).

Technical Writing and Editing: Source Literature; Elementary Textbooks

DUANE ROLLER

Wabash College, Crawfordsville, Indiana

SIR James Barrie is reported to have said that "the man of science appears to be the only man who has something to say, just now—and the only man who does not know how to say it." In admitting that scientists really do have something to say, Barrie probably voices a minority opinion among men of letters, many of whom seem to want to perpetuate the conflict of long standing between literary scholarship and science.¹ Barrie's admission is especially significant in view of the peculiar character of his genius, and thus his charge that the scientist is unable to say what he knows should be given considerable weight.

Of course, we realize that Barrie had in mind the scientist in his relation to the layman. My position will be that one good way to approach this question of the general dissemination of scientific knowledge is first to be clear on how we communicate such knowledge among ourselves. To be clear on this is, of course, also useful for the progress of the sciences themselves. Not only is communication indispensable, but the symbology and the framework of language that we employ for this purpose are the very tools with which we think.

SOURCE LITERATURE

We will start then with the sort of writing that is most peculiar to a science: the original, purely technical paper and treatise. This archive literature is so obviously effective, especially in the physical sciences, that we can be confident that nowhere else is knowledge—even the most complicated—recorded and transmitted with more exactness and clarity. The physical scientist has done more than anyone else to develop and preserve the distinction between the *referential* and the *emotive* functions of language—a distinction that has served to put the language of the physical sciences at one end of a scale, with poetry at the other end. One should add that this reference to a scale serves merely to bring out a difference in the linguistic mediums employed by science and poetry. It does not imply, for instance, that the function of a science

is solely to communicate referential meaning. The point to be noted here is that in developing a sharp distinction between referential and emotive functions of language, the scientist has made a major contribution to communication.

No one has succeeded better than the scientist in subordinating language to thought. Yet, since language in one form or another is indispensable, we have become conscious of its dangers and have done much to safeguard ourselves from them. We have gone furthest in freeing ourselves from what Stuart Chase has called the "tyranny of words." Our technical terms and other symbols are relatively unique, and no one is more conscious than we of the *meaning* of symbols. The important influence of the sciences, especially theoretical physics, on semantics adds still more to the contention that our contributions to language have been fundamental.

Although these various contributions are familiar to us, we should allow ourselves to be more impressed by them. We should recognize that many problems of language lie within our province and should feel freer than we have in the past to participate in language reforms when they appear to be needed. Moreover, if the sciences have brought about marked improvements in certain types of written communication, we should be clear on how it has been done, for this will suggest ways to improve all kinds of technical and semitechnical writing.

By examining many obviously good research papers, we can easily find some of the general reasons for their functional excellence. *First*, and the most evident, is the fact that the author of a first-rate paper is thoroughly steeped in his subject. *Second*, he knows his readers—the extent of their knowledge of the subject and its terminology, what they need to be told and need not be told. *Third*, he restricts himself, mainly, or entirely, to referential language, so as to convey, as nearly as possible, the same meanings to all trained readers; moreover, he knows that the use of emotive language, especially to gain an important point, will usually be resented by the specialist, who needs no program notes to tell him when and what to feel. *Fourth*, the author of a good technical paper is sensitive to questions of terminology and to semantic difficulties,

¹ For an illuminating and constructive discussion of this conflict, see C. I. Glicksberg, "Literature and science: a study in conflict," *Sci. Mo.* 59, 467 (1944).

although he may not recognize the latter as such. *Fifth*, he is relatively unhampered by the need for creating patterns of writings. Not only do the preceding papers in the field serve as models, but such standardized devices as mathematical operators, graphs, tables, and abbreviations are available for presenting complicated and detailed information economically and understandably, and the experienced writer uses them automatically, without having to divert attention from the central and novel features of his work.²

Information of this sort on how technical writing is facilitated may be made the basis of rules for technical composition, extended to cover such familiar topics as unity and coherence, conciseness and concreteness, diction, and so forth. Such guides should be supplied even to beginners in a science, for good writing habits acquired in undergraduate laboratories and classes would save a person much time and effort in later years. Undergraduates should be taught from the first how to write progress and summary reports, and how to prepare manuscripts in a form suitable for publication.³ Graduate students engaged in experimental research should learn the value of preparing progress⁴ and summary reports during the course of their work. Clearer ideas will be obtained of the directions that the work is taking and of the parts that should be intensified, and the writing of the final thesis will be a much simpler task. Discrepancies in results and weaknesses in supporting data should be detected during the course of

the experimental work, and not while the student is writing the thesis, when it might be difficult to obtain additional data.

Obviously a report that represents little more than a day-by-day record of the author's work and even his learning processes should not be submitted for publication.⁵ Such a paper is almost impossible to edit, for it should be entirely recast. The best advice to give the author is to *first prepare the abstract and then write a paper that will support it*. This might be a good rule for anyone to follow.

In evaluating technical writing, one must remember that an occasional paper may not be very good but still may be important. This can happen when a discovery is highly original and perhaps unorthodox in its implications, for then the author must have the courage to publish promptly, before he has thoroughly worked out the development in his mind or has found the most effective way to present it.⁶

Errors that should cause us real concern are the inaccurate data, mistakes in calculations, and even misconceptions that are often the result of mere carelessness or laziness on the part of an author; and the literature contains more of them than most readers may suspect.⁷ It should be remembered that in this country the editors of the physics periodicals must carry on their editorial work more or less as an avocation, and that the referees of papers submitted for publication receive no fees and often lack the time that would be needed to check every detail. Under these circumstances, the author and his institution must assume the major responsibility

² Hence, the desirability of selecting the most intelligible and economical forms for all such devices and then, invariably, using them. Making the selections is a relatively simple task, but some committees working on it have made unsatisfactory recommendations because of failure to base their work on acceptable principles of selection. Such principles—for example, those to be used in selecting abbreviations for the names of physical units—obviously should be framed, not arbitrarily, but in the light of all other related practices in physical science. For an example of a committee report that appears to be generally satisfactory, see *Engineering graphs for publications* (American Society of Mechanical Engineering) and the review of this report in *Am. J. Phys.* 11, 230 (1943). Also see the AAPT committee report, "Proposal to standardize letter symbols," *Am. J. Phys.* 8, 300 (1940); the list of symbols and operators presented in this report has now been revised in the light of criticisms received by the committee and will soon be issued as a publication of the American Standards Association. Several engineering committees have published lists of abbreviations for the names of physical units, but the ones that I have seen are not very satisfactory for the purposes of physical science.

³ Concise instructions for contributors to the *American Journal of Physics* will be found on cover 2 of the February 1945 issue.

⁴ A suggested outline for weekly reports is given in a note, "Supervision of student research," *Science* 75, 285 (1932).

⁵ Franklin, in a letter to James Bowdoin, wrote: "By throwing our occasional thoughts on paper, we more readily discover the defects of our opinions, or we digest them better, and find new arguments to support them. This I sometimes practice; but such pieces are fit only to be seen by friends." This quotation and that in footnote 6 are taken from I. B. Cohen, *Benjamin Franklin's experiments* (Harvard Univ. Press, 1941), pp. 319 and 279, respectively.

⁶ "These thoughts, my dear friend, are many of them crude and hasty; and if I were merely ambitious of acquiring some reputation in philosophy, I ought to keep them by me till corrected and improved by time, and farther experience. But since even short hints and imperfect experiments in any new branch of science, being communicated, have oftentimes a good effect, in exciting the attention of the ingenious to the subject, and so become the occasion of more exact disquisition, and more complete discoveries, you are at liberty to communicate this paper to whom you please; it being of more importance that knowledge should increase, than that your friend should be thought an accurate philosopher."—*Letter from Franklin to Peter Collinson*.

⁷ See, for example, the article in this issue by R. T. Birge, who has rendered a great service by repeatedly calling attention to this fact. Also see N. E. Dorsey, *Trans. Am. Phil. Soc.* 34, 1 (Oct. 1944).

for the soundness of a paper, and for the accuracy of all data, derivations, calculations and literature references in it. To eliminate all imperfections is difficult, as any experienced writer or editor well knows; indeed, it is not uncommon to receive criticisms of a published paper or book, and to find that the critic himself has made some slips. Nevertheless, if a reader detects an error that materially affects the usefulness of a paper, he should call attention to it and supply the necessary corrections. For an editor or author to receive such criticisms may not be pleasant, but this cannot be helped.

As for mere typographical errors, it is hard to understand why any author, after spending months in research and study, and many hours in writing, should fail to take the few additional minutes needed to read the stenographer's typescript against his original copy. Typographical errors in numerical data or underived formulas, for instance, may escape detection during editing, and are revealed only after other workers in the field have experienced inconvenience and loss of time in trying to use the results. If an author fails to check his typescript for errors, the chance is small that he will properly attend to the galley proof which is sent to him at considerable expense. Fortunately, the number of mistakes made by our own highly competent printers, the Lancaster Press, is almost negligible in comparison with the number found and corrected in typescripts submitted for publication.

There have been complaints that many papers published in journals such as *The Physical Review* are now so specialized and condensed that only experts in the respective fields can get much out of them. This change in the character of our archive journals is to be expected, for it merely reflects the enormous increase in output, complexity and quality of physical research in this country. Even *Reviews of Modern Physics* has become quite specialized, but nevertheless supplies valuable survey articles that are useful even to specialists themselves. Such survey, or summary, papers will be especially needed after the war, as soon as restrictions are lifted on those projects that have involved fundamental advances in physics.

There still remains the somewhat neglected task of keeping all physicists informed of trends in the science as a whole. In the AMERICAN JOURNAL OF PHYSICS we have made an effort to obtain survey articles of intermediate grade that can be read with profit by anyone having a good knowledge of general physics. An attempt has also been made to assist the authors by supplying them with information which we have collected on the technics of writing such articles.

Our success has been moderate. The easiest thing we could do would be to yield to the pressure unconsciously put on us by writers to raise the grade of difficulty. Some authors fear the opinions of other workers in the same field; in this respect they are like a graduate student giving his first colloquium talk, whereas their real function should be more like that of an experienced lecturer to graduate students, who lets himself be as elementary and frankly pedagogic as the occasion demands. The ability to be truly elementary in dealing with complicated material is one sign of intellectual maturity.

To produce a general paper that will be really useful to the whole profession is something like writing a first-rate chapter to supplement a textbook of about senior or first-year graduate grade. Looked upon in this way, the general article ceases to be hack work of little significance either to specialist or to general reader, and becomes a creative job that can be very interesting. Many physicists might find such writing surprisingly stimulating, and it could supplement rather than make inroads into their more serious creative work.

Need for an Auxiliary World Language

To return to *source* literature, we should face one problem of which we may be too little conscious. This is concerned with the common language that is needed if the results of research are to be made readily available to workers of all nations. If this problem has not been critical in the past, it is because nearly all physicists, regardless of their nationality, have been trained in universities where either English, German or French is the primary language. But as scientific activities become more intense all over the world, more and more workers will be trained at home—in Russia, China, Japan, Latin America—and we may find ourselves partly cut off from one another by language barriers. For this reason, and also to promote economy of effort in learning languages, we will ultimately be forced to adopt some one international, auxiliary language of science. This problem clearly is within the domain of science itself and can be solved more rapidly and effectively by scientists than by any outside group. Indeed, part of our language—the mathematics—already is international, and this is also true of units of measurement, time conventions and the hundreds of newer technical terms that are almost alike in the chief Western languages.

The history of attempts to develop an international language for science dates at least from the seventeenth century, but it should suffice here to mention two recent studies. In 1921, a committee of the British Association gave unanimous approval to the desirability of having an international, auxiliary language, and, on the basis of expert evidence, reached the conclusion that an invented language constructed on sound principles—for example, Esperanto—is more practicable than either a dead language, such as Latin, or a modern language.⁸

In the same year, 1921, a similar committee of the American Association for the Advancement of Science asserted that the idea of an international, auxiliary language deserved more interest and support than it had received in the past, and recommended: (i) the clearing up of the facts and theories underlying the choice and development of such a language; and (ii) international agreement on these facts and theories and on some program for action.⁹

Perhaps these two committees would now make somewhat different recommendations in the light of events since 1921. For one thing, the demand for a world language has become more emphatic; indeed, a recent British Association committee on postwar education has recommended that every university should require its students to be able to speak and write an international language.¹⁰ Much has also been done to clear up the underlying facts and theories mentioned by the American committee. As for this committee's recommendation concerning *international* agreement on some language, such action surely would come too slowly to be of practical value in the immediate postwar years.

If we take all changes of the past 20 years into account, the present answer to the question of what the international language should be, clearly is *English*. Even before the present war the tendency was toward English in international commerce and trade. Today, war devices and materials originating in English-speaking countries are going to every part of the world, and thus numerous English technical and vocational terms are being added to languages that have no equivalent or substitute for them.

Compared with French or German, English has certain characteristics that make it the best

language of the three for the sciences. Without bothering about the question of whether English is the ideal science language for all time to come, we should accept the fact that *it is the one language that can be gotten into general use almost immediately*. If this is not done, it will be the fault of scientists who write in English. We must take the initial steps—absurdly simple steps, but with possible eventualities that are really immense.

The first step is merely that authors and editors of *abstracts* and *archive articles*, nothing else, become thoroughly conscious of all readers in their own field, but in other countries, whose reading knowledge of English will be only moderate and not comparable with that of a physicist trained in a British or American university. This simple step can be taken immediately, and, even if nothing else is done, will accomplish a good deal.

But soon there will be a demand for a common vocabulary upon which all authors and readers can rely. Such a vocabulary, which must be developed by experts, is already available as part of the system known as Basic English.¹¹ As Hugh Walpole¹² has pointed out, the 850 words in the General Basic vocabulary have been tested and studied as no other words in any language were ever studied before. Moreover, the system, with its simplified grammar, has now been extended to provide basic vocabularies for the sciences, including mathematics.¹³

Certainly all *abstracts* of technical papers could immediately be written in Basic English, and here the editors of our journals can take the lead, if they will. As Walpole says, one must not try to swallow the system in one gulp; to learn it takes a whole day.

The critics¹⁴ of Basic English as a *general* world language doubtless have some good arguments, though not too many, else they would less frequently resort to mere ridicule and to reminders of possible sacrifices, linguistically in this case, in giving aid to foreigners. However, our interest here is not in the general use of Basic English, but in its adoption as an *auxiliary*

¹¹ C. K. Ogden, *The system of Basic English* (Harcourt, Brace, 1934). For a brief description of the system, see reference 12.

¹² H. Walpole, *Semantics—the nature of words and their meanings* (W. W. Norton, 1941), chap. 9, "Applied semantics."

¹³ C. K. Ogden, *Basic for science* (Kegan Paul, 1942).

¹⁴ For example, Rose Macaulay, "Against Basic English," *Atlantic Mo.* 173, 58 (Apr. 1944).

⁸ R. Gregory, *Nature* 150, 622 (1942).

⁹ S. W. Stratton, *Science* 55, 166 (1922).

¹⁰ Reference 8.

world language for the sciences. If there is any valid reason against such adoption, I am not aware of it.

ELEMENTARY TEXTBOOKS

There are, I believe, more elementary textbooks in physics than in any other field except English. Of all scientific writings, such textbooks appear to be next in importance to purely source literature. Moreover, if these books are of high quality, authors of intermediate and advanced textbooks will be forced to improve their products.

Probably every elementary book has a number of special features, some of them quite original, that have helped to enrich various aspects of teaching. Taking these many good features for granted, I will generalize numerous specific criticisms of various books so as to indicate some ways in which *improvement* seems to be possible.

(i) There can be a much better integration than heretofore in the several traditional branches of elementary physics.

(ii) The amount of material covered should be greatly reduced, by courageous elimination of selected topics and by a better correlation of factual material with a relatively few general principles.

(iii) We should try to return a little to the easy and fluent style of some earlier textbooks written by excellent physicists who were also men of broad education and wide reading.

(iv) Nearly all books need much more careful editing for uniformity of terminology, notation, abbreviations, proper numbers of significant figures, and other similar recurring features. Such uniformity, which is easy to attain, adds materially to orderliness and elegance in a book that is dealing with a science having those characteristics.

(v) The author should have the patience to subject the meaning of every word in his manuscript to the most careful analysis. If he lacks the necessary sensitiveness to language, he should employ someone to do this for him. Because it is not always done, we find in books hundred of statements similar to the following:

The release of the pressure is a linear function of the time.

The phase angle is the angle subtended at the center of the circle of reference by the motion of the point along the circle.

The heat of vaporization of a liquid is the number of

calories required to vaporize a unit mass under normal pressure without changing its temperature.

The current flowing in an a.c. circuit equals the quotient of the voltage divided by the impedance.

Such statements are not so much an indication of fundamental misconceptions on the part of the author as of mere careless and uncritical writing and editing.

As for fundamental misconceptions, if a manuscript contains many of them, an attempt merely to edit it is usually a waste of time. Yet, as most of us well know, a few inexcusably faulty manuscripts have gotten by reputable publishers and, what is worse, have in a case or two been almost "best sellers," at least for a time. It seems certain that many teachers fail to read a book carefully before they adopt it. To judge the teachability of a textbook before actually using it with a class is difficult, but at least one can determine whether or not the physics in it is sound.

Earlier I mentioned that one reason for the functional excellence of many research papers is that the author knows exactly for whom he is writing. Perhaps we can see how this affects some of the elementary textbooks that have been designed for particular kinds of students. For present purposes we may divide such books into three groups: (i) books designed primarily for *superior* students who plan to enter a science, or mathematics or high-grade engineering; (ii) books designed mainly for the average engineering, premedical or other technical student; (iii) books intended for the general student who takes physics primarily as part of a liberal education.

A book in the first group is probably the easiest and most enjoyable for a physicist to write. Most of his readers will be students who are capable of becoming creative scientists or creative engineers. The author is dealing with his own kind, and hence knows well their requirements and what he should demand of them.

Of the books in the second group, a number of those designed for the pre-engineering course apparently have proved to be quite satisfactory. The criticism perhaps most frequently heard is that the relative emphasis given to the various parts of elementary physics needs some readjustment in the light of modern engineering practices.¹⁵ As to what type of book is most suitable for premedical students, opinions differ considerably. Certainly it is untenable to as-

¹⁵ See, for instance, R. F. Paton, *Am. J. Phys.* 11, 45 (1943).

sume that a textbook designed specifically for such students will necessarily be superficial and easy. Present indications are that special, basic textbooks will eventually come into use wherever classes are composed solely of students of pre-medicine and closely related fields, that these books will stress fundamental physics and will be of good quality, and that they will be found eminently satisfactory when used to maximum advantage by physics teachers who have a genuine interest in premedical training.

A point that applies to books in both the second and third groups concerns the level of the general, or framework, vocabulary employed. We merely handicap our teaching if we try to compel the average student to increase his ordinary vocabulary appreciably in order to be able to read the textbook. We make a substantial enough contribution to his understanding of language by insisting that he gain facility in the use of mathematics, which is itself a language, and that he learn to define and use many new technical terms with a precision that is seldom required of him elsewhere. He should not, in addition, be asked to look up definitions of words in definitions.

The third group of books, those for liberal arts students, includes many that contain good, sound physics and are carefully written. Most of the topics are treated with great simplicity and, often, with ingenuity. Indeed, it appears that some authors become so engrossed in the difficult task of making the physics clear and understandable that they lose sight of why liberal arts students should be taking physics. It is not surprising, then, that their books are most successful in classes composed mainly of physical science majors.

Attempts to make physics more attractive to nonscience majors, and also to adjust the teaching of it to the capacities of our growing student bodies, have, in recent years, engaged the attention of a number of authors. Some of the resulting books are intended for college survey courses, which, at the present time, appear to have a useful function. However, it seems certain that much of the simple phenomenology dealt with in many survey courses will eventually find its rightful place in the secondary school, or even in the grades. Moreover, even if college survey courses do prove to have a lasting function, it is highly questionable whether they alone can serve to give able students the kind

of training that makes physics most valuable educationally.

These able students—roughly the upper quarter of those majoring in the humanities, government, economics, the creative arts—need to acquire the attitudes, understandings, modes of thought and procedures that physics is in an almost unique position to give them. But such training, if it is to be functionally effective, must come from extensive practice in formulating and attacking problems, in thinking reflectively, and with mathematical precision, in gaining real understanding involving drastic changes in beliefs and behavior.

Now, no sensible person will deliberately involve himself in a rigorous program of this sort unless he is sure that it is significant for him, that the insight and training afforded are almost indispensable for his purposes. Certainly it is possible to make physics highly significant and almost indispensable for high-grade people operating in a culture that is coming to be predominately scientific; but the opinion prevails among most nonscience students that we have not done it.

Although the needed reformulation, when completed, will be drastic,¹⁶ the first steps have already been taken by a few—a very few—authors. If they have gone only a little way, it may be partly because they, and their publishers, know that teachers hesitate to adopt a new book if it differs by more than about 15 per cent from existing books. Another deterrent is the enormous amount of preliminary work in collecting and developing material before each new step can effectively be taken. This preliminary work involves the collection of pertinent, reliable material from a variety of sources—philosophical, historical, psychological, social, economic, and so on.

Because the physics with which this material must be integrated will be just as fundamental and sound as that in any traditional book, it is primarily the physicist who must write the new book, for the same reason that he must write the physics textbooks for engineers. If he is writing for social and human, instead of physical, engineers, this should not bother us. The sciences have contributed greatly to an understanding of human conduct and of how social institutions

¹⁶ The character of this reformulation is mentioned in more detail in my article, "The role of the sciences in general education," *Am. J. Phys. (Am. Phys. T.)* 6, 244 (1938), esp. pp. 247-251.

should operate. Indeed, we should never forget how it has been the sciences that have made possible, for the first time in history, a civilization in which all the people *can* be free from hunger and poverty, from superstition and fear—a civilization in which, for the first time, democracy can flourish on a large scale.

The scientist has been criticized for failing to put scientific knowledge into such form that it can be effectively assimilated into the culture which *he* initiated but which we are entering so slowly and indeed painfully. But the truth is that the scientist is not the only one who has failed. Before the scientific outlook can ever become really functional in the great mass of people, the *human and emotional significance* of all kinds of scientific knowledge must be formulated; and this latter is primarily the task, not of science, but of creative literature and art.

This makes the cultural responsibility of the physicist much less formidable. His function in this respect is not that of a Dostoevski or a Ruskin or even a Jules Verne. He should do for society what no one else can do: render

physical knowledge so significant for *very able* laymen, that large numbers of them will be willing to undergo the training necessary if they are really to assimilate the scientific attitude. In bringing this about, the liberal arts textbook can be a most important factor, and possibly it is through such books and the teaching from them that physicists can make their greatest cultural contributions.

* * *

Barrie was right: the man of science is the one who has something that needs to be said; but the linguistic medium and modes of interpretation peculiar to the sciences and so necessary for their progress are only partially effective when it is the people that are to be reached. Creative literature and art must complete the interpretation, and to help make it possible for literary scholars, artists and other capable non-scientists to utilize their special talents in such directions is one of our important tasks. There need be no conflict between science and letters; both can and must learn to work harmoniously toward a common purpose.

Painting with all its technicalities, difficulties and peculiar ends, is nothing but a noble and expressive language, invaluable as the vehicle of thought, but by itself is nothing.—JOHN RUSKIN.

*Science! true daughter of Old Time thou art!
Who alterest all things with thy peering eyes.
Why preyest thou thus upon the poet's heart,
Vulture, whose wings are dull realities?
How should he love thee? or how deem thee wise,
Who wouldst not leave him in his wandering
To seek for treasure in the jewelled skies,
Albeit he soared with an undaunted wing?*

*Hast thou not dragged Diana from her car?
And driven the Hamadryad from the wood
To seek a shelter in some happier star?
Hast thou not torn the Naiad from her flood,
The Elfin from the green grass, and from me
The summer dream beneath the tamarind tree?*

EDGAR ALLAN POE, *Sonnet: To Science*

To write well, even to write clearly, is a woundy business, long to learn, hard to learn, and no gift of the angels.—JOHN GALSWORTHY, Foreword to HUDSON'S *Green Mansions*.

Variation of Size and Charge of Positive Lichtenberg Figures with Voltage

JOHN ZELENY

Yale University, New Haven, Connecticut

WHEN an electrically charged conductor is momentarily brought into contact with a dry glass plate which is resting on a grounded sheet of metal, no appreciable amount of charge is transferred to the glass surface unless the potential of the conductor is sufficiently high to initiate disruptive discharges through the air. The channels of such discharges pass close to the glass surface, and the charge transferred to the glass is distributed over a limited area of the surface in characteristic line patterns which are known as Lichtenberg figures.¹ These figures may be made visible by sifting a mixture of finely powdered sulfur and red oxide of lead through a cloth bag onto the glass after the discharge has passed.

The measurements on these discharges which are here described were made in connection with work on condensers with removable coats,² where similar discharges may occur in the air spaces between the coats and the dielectric, as is evidenced by the Lichtenberg figures which may be developed on the surface of the dielectric after one of the coats is removed.

One object of the experiments was to find the relation between the size and charge of Lichtenberg figures as a means for estimating the charge that passes under given conditions from the charged coat of one of the aforementioned condensers to the surface of the dielectric during the process of charging. Since discharges to the dielectric occur simultaneously from both coats of the condenser, a direct measurement of the charge on the whole dielectric gives only the difference between the opposite charges delivered to its two surfaces.

Instead of getting directly the relation between the size and charge of the figures, it was believed to be of added interest to find separately how the size and charge of the figures depends on the voltage required to produce them, and from these data to obtain the desired relationship. The number of main branches in the Lichtenberg figures was found to be an added factor of some importance.

The apparatus employed was the same as that used in the condenser experiments.² The glass

plates were placed on a grounded metal disk, 15 cm in diameter, and the conductor that carried the charge to the glass was a universal discharger, such as is used in electrostatic experiments, consisting of two metal rods pivoted together, each having an ebonite handle at one end. Contact between the conductor and the glass was made either by means of a spherical ball of 15 mm diameter attached to one arm of the discharger or by the rounded end of a wire of 1.6 mm diameter fastened to the other arm. Between readings the charges on the glass plates were neutralized by holding the plates above the flame of a Meker burner. The work was done during the winter, when the air humidity is low.

CHARGES ASSOCIATED WITH LICHTENBERG FIGURES

The charge transferred to a glass plate during contact with the conductor depends not only on the potential of the conductor and the thickness of the glass but to a marked extent upon the sign of the charge used. The following example illustrates the influence both of the thickness of the glass and the sign of the charge on the amount of charge transferred. The discharger (capacitance 7.1 cm) was charged to 6760 v and hence initially carried 160 esu. A single tap of the ball on one arm of the discharger against a glass plate 2.95 mm thick transferred to the latter, on the average, 26 esu when the discharger was charged positively and only 15 esu when it was charged negatively. The corresponding numbers obtained with a glass plate 1.7 mm thick were 44 and 24 esu, respectively. The fact that a larger charge is transferred to the glass from the conductor when the latter is charged positively than when it is charged negatively doubtless results from the known differences between the disruptive discharges in the two cases. Since the potential of the conductor was reduced considerably by its loss of charge to the glass plate, the losses recorded would have been larger had the capacitance of the conductor been larger or if its potential could have been maintained constant during the operation. The method used precluded the fulfilment of either of these conditions. The charge transferred to the glass was

¹ For a historical review see K. Przibram, *Handbuch der Physik* (Springer, Berlin, 1927), vol. XIV, p. 391.

² J. Zeleny, *Am. J. Phys.* 12, 329 (1944).

found from the difference between the charge on the conductor before the contact was made and that remaining after the contact, both quantities being measured on a galvanometer calibrated for ballistic use. The first charge could also be computed.

The measurements of the size of the Lichtenberg figures had to be confined to positive figures since the figures for negative charges are less regular in shape and their details are not well shown by the powder method here used to develop them. Hence the charges also were only measured for positive figures. Sufficiently extensive results to warrant reporting were obtained only with glass plates 1.7 mm thick.

The results are shown in Fig. 1, which gives the relation between the initial potential of the conductor and the charge delivered to the glass during a momentary contact between the two. Curve *A* represents the data obtained when the terminal of the conductor touched to the glass was the rounded end of a wire of 1.6 mm diameter, and curve *B*, when a sphere of 15 mm diameter was used.

The dispersion of the individual observations is much larger than the observational errors warrant. These divergencies result most probably from the fact that the figures obtained with the same voltage did not always have the same diameter or the same number of branches. No explanation for these apparently fortuitous variations was discovered. A search for a possible surface effect was made by taking readings in succession on the same day with a glass plate

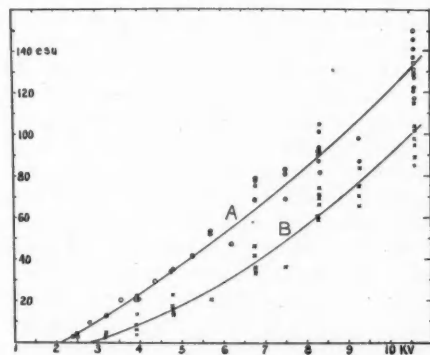


FIG. 1. Relation between voltage used and the charge on positive Lichtenberg figures produced on a glass plate 1.7 mm thick by contact of a charged conductor (capacitance 7.1 cm) through (A) the rounded end of a wire of diameter 1.6 mm and (B) a spherical ball of diameter 15 mm.

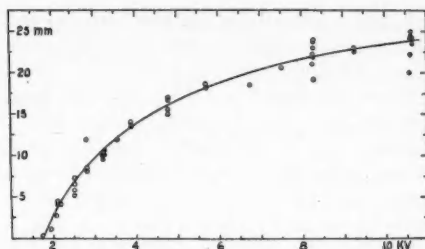


FIG. 2. Relation between voltage used and the diameter of positive Lichtenberg figures produced on a glass plate 1.7 mm thick by contact of a charged conductor (capacitance 7.1 cm) through the rounded end of a wire of diameter 1.6 mm.

that was subjected to different treatments. The glass was first used after it had stood idle for three days, next after it had been heated in the hot gases above a burner, then after it had been chemically cleaned and the final distilled water removed only by means of a towel, again after it had been dried over a hot radiator and used while quite warm, and finally after being again heated over a burner. All of the results were in substantial agreement.

No allowance has been made in the results of Fig. 1 for any possible losses of charge by the conductor owing to conduction of the glass plate or of the ebonite handle by which the conductor was supported. Measurements made with the conductor held in contact with the glass for 5 sec, at times gave no appreciable increase in the charge lost by the conductor over that lost during a momentary contact and no change in the Lichtenberg figures was noticeable, but more frequently such a long time of contact not only increased the loss of charge considerably but the figures obtained were both appreciably larger and composed of more branches. However, considering the short time taken ordinarily to make an experiment, it is not believed that conduction losses ever affected the results by more than 3 percent.

Figure 1 shows that once the limiting voltage for the initiation of disruptive discharges is reached the charge received by the glass increases at a somewhat larger rate than does the voltage. Owing to the smaller curvature of the sphere, the positive discharges from it began at a potential about 1000 v higher than was required for the wire-end contact, and the charges delivered by it to the glass remain lower throughout the whole range of potentials used.

The only previous measurements of charges

carried by Lichtenberg figures are those of Przibram,³ and these were made at higher voltages and under rather different conditions than those here used so that the two sets of results are not directly comparable. He also found that the charges carried by the positive figures are larger than those carried by negative figures.

DEPENDENCE OF SIZE OF POSITIVE LICHTENBERG FIGURES ON VOLTAGE

The way in which the diameters of positive Lichtenberg figures depend on the voltage used to produce them is shown in Fig. 2. The momentary contact between the charged insulated conductor and the glass plate (1.7 mm thick) was made with the rounded end of the wire of 1.6 mm diameter.

For the higher of the voltages here used the positive figures nearly always had a circular outline and, while the individual tracks were not all of exactly the same length, the diameters of the figures could be measured with adequate accuracy. Below 3000 v, the figures varied from mere dots and lines about 1 mm long to unsymmetrical figures with three to five short straight arms. Very rarely, at somewhat higher voltages, figures appeared not only with six or eight equal straight arms but with these also separated by remarkably equal angles.

Figure 2 shows that for the smallest potentials the lengths of the channels increase rapidly with voltage, and then, at higher voltages, the rate of increase becomes smaller and nearly linear. Similar characteristics were obtained for the size-voltage relation by v. Bezold⁴ and Mikola,⁵ who found a truly linear relation between the two quantities for the higher voltages up to 50 kv.

RELATION BETWEEN SIZE AND CHARGE OF POSITIVE LICHTENBERG FIGURES

The average results given by the curves in Figs. 1 and 2 for the variation of the charge and size of Lichtenberg figures with voltage for a glass plate 1.7 mm thick are combined in Fig. 3 to show directly the relation between the size and charge of the figures. For the larger sizes, the charge increases so rapidly with size that in this range a large error could easily be introduced in attempting to get the charge associated with a figure from its size.

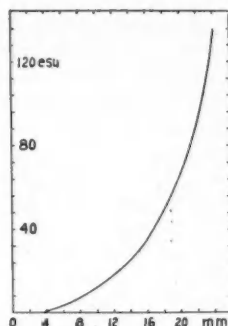


FIG. 3. Relation between charge and diameter of positive Lichtenberg figures produced on a glass plate 1.7 mm thick by contact of a charged conductor (capacitance 7.1 cm) through the rounded end of a wire of diameter 1.6 mm.

As was previously mentioned, the variations in the charges obtained at any one voltage are probably caused by differences in the length and number of the separate discharges appearing in the figures. Some measurements were therefore made to learn whether the charges associated with the figures obtained for different voltages are proportional to the combined length of all of the separate discharge paths appearing in the figures. The total charge on an individual figure was measured and the length and number of its separate main discharges determined, and from these values the average charge per unit length was found. The length of each discharge was taken as the radius of the figure obtained with the small wire terminal on the conductor, disregarding any crookedness in the path. The complexity of the figures obtained with the higher voltages presents some difficulty in determinations of the number of separate discharges involved in a given figure. A track with all of its bifurcating branches was counted as a single discharge except where the main channel divided near its origin into two nearly equal parts, in which case it was counted as two. The number of discharges in a single figure varied from about 8 at 3200 v to about 35 or 40 at 10,600 v. The method being described would probably be inapplicable to voltages higher than the one last named, because even at that voltage the figure occasionally changes from a symmetrical form to one in which the main channels group themselves into a few heavy clusters which only break into numerous fine forks near their ends.

Measurements were made as described, over the voltage range named, mainly with a glass

³ K. Przibram, *Wien. Bericht.* **128**, 1203 (1919).

⁴ W. v. Bezold, *Pogg. Ann. d. Physik* **140**, 145 (1870).

⁵ S. Mikola, *Phys. Zeits.* **18**, 158 (1917).

TABLE I. Average charge per unit length on positive Lichtenberg tracks.

Dielectric	Thickness (mm)	Number of readings*	Charge (esu/cm)
Glass	1.7	17	2.9 ± 0.06
Glass	2.95	5	2.5 ± 0.14
Ebonite	1.7	3	2.3 ± 0.10
Ebonite	6.6	3	2.3 ± 0.15

* What is designated here as one reading consisted of the average of measurements made on at least three successive discharges to different parts of a plate.

plate 1.7 mm thick, but a few readings were also taken with a thicker sheet of glass and with ebonite of two different thicknesses. The results obtained for the average charge per unit length are sufficiently concordant to give some merit to the method used, and a summary of these is given in Table I.

The tracks on the ebonite plates were somewhat longer and were less numerous than those obtained for the same voltage on glass plates, and the average charge per unit length is seen to be smaller. The results shown make it possible to get an estimate of the charge not only on a complete figure but also that on each single track of discharges that occur along an insulator from the edge of an extended charged conductor brought into contact with it.

To illustrate the use of the data, consider the case of the dissectible condenser cited in a previous paper.² The coats of the condenser were disks of 15 cm diameter, and the dielectric was a glass plate 2.6 mm thick. After the condenser had been charged to +9000 v, the upper disk was removed and, on dusting the glass plate with powdered sulfur, some 500 Lichtenberg figures appeared on the area which had been underneath the disk, each of which had a diameter of about 6 mm and consisted of five arms. In addition, this whole area was surrounded by about 470 radial lines of an average length of 8.5 mm, representing discharges from the edge of the disk. The total length of all of the discharges was thus about 1150 cm and, using the average charge as 2.6 esu/cm, this gives for the total charge transferred to the dielectric approximately 3000 esu.

SPECIAL TOPICS

Which type of Lichtenberg figure would be obtained if conditions were reversed and a

grounded metal sphere were to touch a highly charged glass plate? Experiment showed that the figures which appeared were always of a sign opposite to that associated with the charge on the glass surface. Owing to the conductivity of the sphere, the density of the charge induced on it at the part nearest to the glass is larger than that on the glass itself, and hence the discharge can start more readily from the metal surface. Obviously then, when a crackle is heard as a finger is approached to a highly charged insulator, the discharge originated at the finger and not at the surface of the insulator.

Why is it that the discharge paths in air, when near the surface of an insulator as exemplified by Lichtenberg figures, are so much longer than the distance that could be bridged by the same voltage in the absence of the insulator? The long sparks that can be obtained between two electrodes along the surface of an intervening sheet of glass have, at times, been erroneously ascribed to a conducting film on the glass surface. In the modern theory of spark discharges, photons from one part of a discharge are called upon to help propagate the discharge to points ahead by liberating electrons there from some of the gas molecules. The presence of a glass surface may, therefore, facilitate the passage of a discharge by enhancing the supply of electrons in the crucial region since the solid state offers a greater concentration of molecules there for the photons to strike, and possibly, too, because electrons may be ejected more readily from glass than from air molecules. The difference in the charge per unit length that was found in Lichtenberg figures on glass and on ebonite may be caused, at least in part, by a difference in the ease with which electrons are ejected by photons from the two substances.

NOTE. In a recent paper to which reference has been made,² all of the voltages given above 2000v were read directly from an electrostatic voltmeter which a subsequent calibration has shown reads too high for a large part of its range. These voltage errors do not affect any of the general results in the paper. The corrected capacitance of the universal discharger (p. 337) is 7.1 cm and that of the upper condenser plate (p. 338) is 5.6 cm. The corrections to be applied to the voltage readings are as follows: -200 at 2000, -550 at 3000, -800 at 4000, -1060 at 5000, -1240 at 6000, -1300 at 7000, -1320 at 8000, -680 at 9000, and +600 at 10,000.

An Apparatus for Demonstrating Standing Waves

MARVIN J. PRYOR*

MOST students come rather quickly to a recognition of what is meant by standing waves, but they are unusually slow in reaching an understanding of the manner in which they are produced. By means of the device shown in Fig. 1 this understanding is attained much more quickly, for the apparatus graphically demonstrates that two identical wave trains traveling in opposite directions really do produce the nodes and antinodes of standing waves.

The vivid manner of adding the separate disturbances caused by the two wave trains will be evident from a consideration of Fig. 2. The two cylindrical drums have horizontal bases, at the adjacent ends, to which are mounted flat gear plates. The opposite ends of these drums are identical sine curves. A single gear, mounted vertically so as to mesh into the horizontal plates, is turned by a crank. This rotates the cylinders in opposite directions about their common central axis. Rollers R and R' are held against the curves by a spring S , so that, when the cylinders are turned, R and R' assume positions which approximate closely the ordinates of the curves. A string of fixed length, attached near the roller R , runs through a pulley in weight I , which serves as an indicator, and thence to a point near the other roller R' . Thus the indicator I is given a displacement that is the resultant of the displacements of R and R' . The result is a most impressive demonstration of nodes, antinodes and any of the intermediate displacements.

When a demonstration of a node is to be made, the frame XY is turned until P points to an "N" on a circular scale on the base. The frame is then clamped in position. When the crank is turned, R and R' each move so as to assume the positions of the ordinates of the appropriate curve. For this setting, simultaneous positions of R and R' are opposite in sign so that the indicator I remains at rest. It is easily observed that the indicator is subject to the displacements of the two like waves traveling in opposite directions, and yet that it remains at rest.

When an antinode is desired, the pointer P is brought to a position "A". The indicator then has a motion the amplitude of which is twice that of either curve. For motions with other ampli-

tudes the pointer is placed as desired between an "A" and an "N".

Time may be "stopped" by holding the crank still. Turning XY about the central axis then

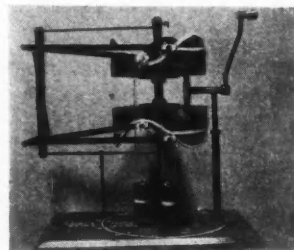


FIG. 1. Photograph of a device for graphically presenting the addition of the ordinates of oppositely directed identical waves.

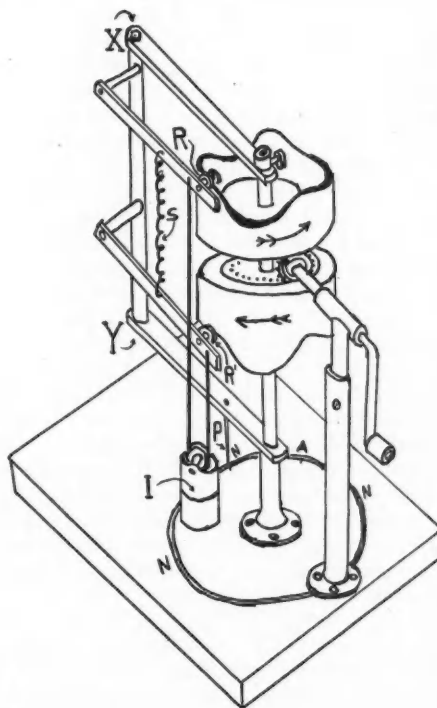


FIG. 2. Drawing of a device which demonstrates the addition of two identical waves that move in opposite or in like directions.

* New York State College for Teachers, Albany, New York.

causes the indicator *I* to assume the positions of successive points along the standing wave for the selected time.

The complete equipment includes two sets of rollers separated by a quarter wavelength in the horizontal direction (Fig. 1). When both indicators are attached, various combinations are readily attained. When one indicator *I* is at a node, the other is at an antinode—a situation that is readily reversed. The two may be between the same nodes and move up and down together, or they may be on opposite sides of a node, in which case one moves up as the other moves down.

The device also affords an excellent demonstration of the combination of identical waves

traveling in the same direction, as in constructive and destructive interference. A collar placed between the cylinders permits lifting the upper one from its usual drive and attaching it to the lower so that the two move together. When the two curves are in the same phase, the indicator *I* moves with twice the amplitude of the curve. When the curves are 180° out of phase, the indicator does not move.

The author is indebted to the Department of Physics of Amherst College for the facilities of its shop in constructing this apparatus. He is particularly grateful to Professor W. W. Stifter for his extended cooperation and to the mechanic, Mr. Warren A. Witt, for many helpful suggestions and much assistance in the actual construction.

Color Spectrograms for Demonstration Purposes

HANS H. KRETSCHMER

Research Laboratories, Western Condensing Company, Appleton, Wisconsin

IN order to facilitate the teaching of the fundamentals of spectrum analysis to the beginner, it seemed advisable to create a more realistic and adaptable tool for demonstrating spectrograms to a large audience. The application of color film suggested itself as the most convenient means for this purpose.

The color photography of spectra can be reduced to a very simple procedure by applying Eastman Kodak 35-mm Kodachrome film. The

(approximately 4000 to 8000Å) into an area of 24×36 mm, the standardized size of 35-mm negatives.

From Figs. 1 and 2 we can see that the spectrograph itself comprises the familiar optical

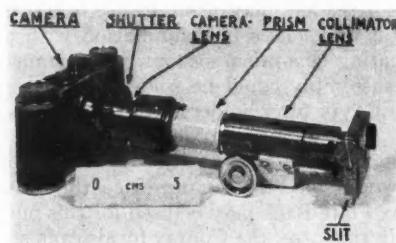


FIG. 1. Photograph of the spectrograph.

cost of this processed film is low, and it may be shown with a standard 2×2 -in. slide projector. Since, for demonstration purposes, no emphasis need be made on high resolving power or large dispersion, a relatively simple spectrograph was built which condensed the entire visible range

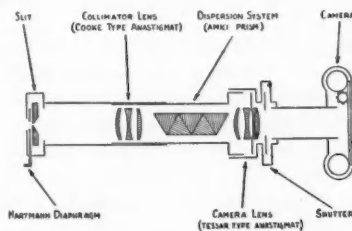


FIG. 2. Diagram of the spectrograph.

units: a collimator, a prismatic dispersion system and a camera. It was built entirely from miscellaneous parts which the author had collected through the years. The dispersion element consists of a direct-vision prism train of the Amici type, incorporating five individual glass prisms cemented together with Canada balsam. The collimator lens is a three-component glass anastigmat of maximum aperture $f:4.5$ and focal length 50 mm. The slit, which is of the bilateral type, has a maximum slit length of 10 mm and is supplied with a Hartmann diaphragm. The slit

Lens Aberrations—A Classroom Demonstration

ARTHUR S. JENSEN

United States Naval Academy,* Annapolis, Maryland

THE present-day widespread interest in photography has resulted in a need for a somewhat more thorough treatment of lenses on the elementary level. Certainly the advanced student should have some concept of the major aberrations of lenses, and even the beginner deserves one simple demonstration of them so that he may better appreciate the lens of his camera. This paper attempts to describe apparatus and procedure for such a demonstration in the classroom. The effects obtainable with the apparatus described herein are on too small a scale and are too faintly illuminated for a lecture room, but the use of an arc lamp and lenses of longer focal length will extend the demonstration to groups somewhat larger than the normal sized class.

First, to review briefly, the six major aberrations may be described by referring to Fig. 1. Here P' is the image formed by a fully corrected lens of the off-axis point object P , and P_1'' and P_2'' are images formed by the lens system when it is similar in position and construction but is not corrected. The two planes shown are the entrance and exit pupils in object and image space, respectively. In a simple lens with no stop, they are coincident with the principal planes when a luminous object is used. When there is a stop in the system, one pupil is the stop itself while the other is the image of the stop formed by the lens. The ray from the object point to the center of the aperture stop is referred to as the *chief ray*.

Spherical aberration may now be described as the variation δx for rays from the same object point traversing the lens system in different zones—that is, for different values of r .

Coma is that aberration which is described by a variation δy for rays from the same object point which traverse the lens system in different zones—different values of r . (This considers only the relatively unimportant meridional rays for the sake of simplicity; each image, formed by rays through each circular zone of the lens,

actually is a circle of which point P_1'' is but the point farthest from the principal axis.)

The aberration of a well-made lens system that is called *astigmatism* exists even for rays for which r is small. This is the condition in which the object P is not imaged as one point image, but as two line images: P_1'' perpendicular to the plane of the paper, and P_2'' in the plane of the paper as shown.

Curvature of field is the name given to the aberration when the image of an extended object departs from a plane. That is, δx , the axial displacement of the image, changes as the lateral distance h is varied for rays from different object points. Actually there is some lateral displacement of the image also, since the chief ray does not intersect the curved surface of the image at the same distance from the axis as it does the plane. Curvature of field is very closely related to astigmatism and is augmented by its presence.

Distortion is the aberration existent when the magnification is not constant for all object sizes. That is, δy , the lateral displacement of the image from its corrected position, changes as the distance h is varied for rays from different object points.

Chromatic aberration. When the images formed by light of different frequencies are displaced from the corrected position P' , the aberration thus described may be thought of as consisting

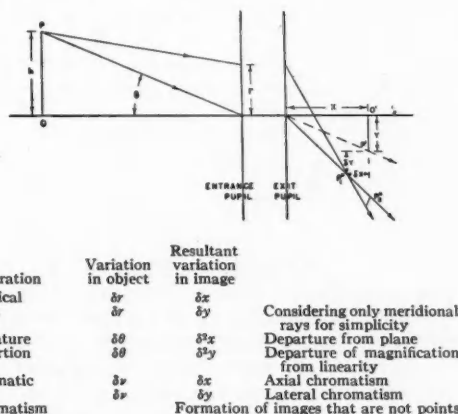


FIG. 1. Object is a point source in the plane PO .

* The assertions contained herein are the private ones of the writer and are not to be construed as official or reflecting the views of the Navy Department or the Naval Service at large.

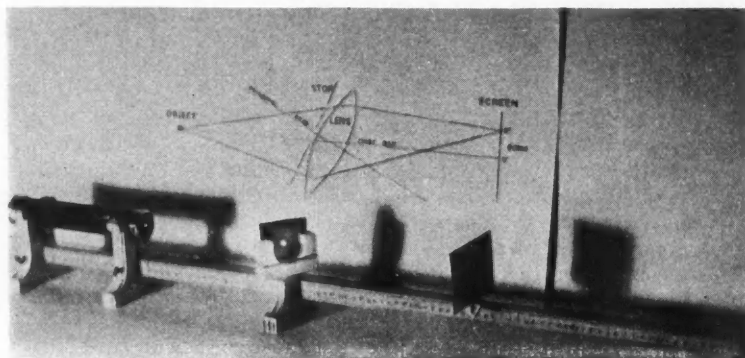


FIG. 2. Portable apparatus.

of two components: *axial chromatism*, the variation δx ; and *lateral chromatism*, the variation δy .

APPARATUS AND PROCEDURE

Essentially all one needs for the demonstration of these aberrations is a source of light (as nearly a point as possible), practically any simple lens, a set of appropriate stops, and a screen. For the classroom, especially if the classes are held in different rooms or even different buildings, a small portable outfit is desirable. This may consist of a flashlight mounted on a meterstick as an optical bench, a lens mount which can be rotated about a vertical axis, and a movable screen (Fig. 2).

The flashlight bulb without a reflector would make a simple "point" source, but with the flashlights available, the reflector could not be removed independently of the bulb. A stop was substituted for the flashlight lens to mask out the light from the reflector. The hole in the stop must be large enough to permit light from the filament to fall on the entire surface of the lens used in the experiment. Some little difficulty was experienced with the flashlight bulbs used since the filament was not strictly a point but a coil of measurable length. However, this was important only for showing astigmatism, and even then a judicious rotation of the filament about the chief ray easily eliminated the difficulty. Usually the individual turns of the coil were used for focusing. Credit is due Dr. R. E. Hitchcock for devising this portable version.

1. *Spherical aberration*. With the principal axis of the lens parallel to the meter stick and the filament source on the principal axis, a sharp image is formed by using only the central zone

of the lens. If now the rays are allowed to pass through all the zones of the lens, the image is blurred. The position of best focus is located with a smaller image distance.

2. *Chromatic aberration*. Attention is called to the color tingeing the image, even for smaller apertures. If a stop having an aperture in the form of a narrow annulus near the edge is placed over the lens, it will yield a hollow cone of light for an axial object point. This appears as a ring on the screen when the latter is on either side of the principal focal point, and the color effects are very obvious. Blue-centered and red-centered images show axial variation.

3. *Curvature*. A sharp image is formed as before. The lens is rotated (and, if precision is desired, moved so that the new object distance p_2 is equal to $p_1/\cos \theta$, where p_1 is the former object distance and θ is the angle between lens axis and meterstick). The object is now no longer on the principal axis of the lens, but has become an off-axis point. Now the screen must be moved in closer to find a sharp image, thus showing the departure of the image surface from a plane.

4. *Astigmatism*. As a matter of fact the only sharp image found is no longer a point but a straight line. Another line image, perpendicular to the first, may be found if the screen is moved still further in towards the lens.

5. *Coma*. Now with the primary or tangential image (the one closer to the lens) on the screen, a stop having three holes in a straight line—a small central hole and two other holes equally spaced from it—is placed on the lens with the line of holes perpendicular to the axis about which the lens was rotated. The rays through

the outer zone holes may be seen to meet each other to form an image farther from the lens axis than the image formed by the rays through the central zone. This may be verified by successively closing off the holes with one's finger, thus showing the variation δy for different zones of the lens. If the stop is removed, the characteristic comet pattern, formed by the addition of the circular images formed by the rays through all the zones, is quite evident.

If again the annular aperture is used, thus allowing the rays to pass through all points of one zone of the lens, the image on the screen will be a closed curve, usually referred to as a *comatic circle*. However, this curve usually is not in one-to-one correspondence with the ideal comatic circle described in the literature since the astigmatism present in the usual simple lens has spread out the conventional circle in a very confused manner. The use of this annular aperture is not recommended for elementary students.¹

6. *Distortion*. Distortion is strongly dependent on the position of the entrance pupil. It can be shown that for a simple lens the distortion is of the pincushion type (greater magnification for larger h) when the entrance pupil is farther from the lens than the object, and of the barrel type (smaller magnification for larger h) when it is between the lens and the object. There is no distortion with a simple lens when the entrance pupil is at the lens (no stop in the system).

If a wire screen is used as an object and illuminated by a diffusing screen behind it, the

entrance pupil is at the lens as before, and no distortion can be detected in the image on the projection screen. However, if an aperture is placed between this object and the lens, the image will definitely show barrel distortion. If the aperture is instead placed between the lens and the image on the projection screen, the image will be afflicted with the pincushion type of distortion.²

It is instructive to repeat this demonstration with a well-corrected camera objective. The author tested an $f/2.5$ Anastigmat from a Foth Derby miniature camera, and the class was effectively impressed by the clarity of the images formed for all possible angles and apertures. Only with the widest aperture and at one particular and rather large angle was a little coma evident.³

The author wishes to express his gratitude to Professor I. C. Cornog, of the University of Pennsylvania, for his very constructive criticism of this demonstration and of the manuscript.

² In this last case the aperture is the exit pupil. The image of the exit pupil formed by the lens is the entrance pupil. When the aperture is outside the principal focus of the lens, as we presume here, the entrance pupil will be located farther from the lens than the object as described in the preceding paragraph.

³ References that are useful in connection with this demonstration are: Hardy and Perrin, *The principles of optics* (McGraw-Hill, 1932), chap. VI; Southall, *The principles and methods of geometrical optics* (Macmillan, 1910); Sutton, ed., *Demonstration experiments in physics* (McGraw-Hill, 1938), p. 390; Glazebrook, *A dictionary of applied physics*, vol. IV, pp. 5-7, 160-171, 396-410; Taylor, *A system of applied optics* (Macmillan, 1906), secs. IV, V, VIII, VIIIA, IX, X; Cox, *Am. J. Phys. (Am. Phys. T.)* **6**, 153 (1938); Dodd and Wiedow, *Am. J. Phys.* **9**, 102 (1941); Monk, *Light, principles and experiments* (McGraw-Hill, 1937), chap. VI.

¹ For an excellent discussion of coma in its various phases, see H. D. Taylor, *A system of applied optics* (Macmillan, 1906).

American Institute of Physics Building Fund

IN financing the new headquarters building of the Institute of Physics, the only things that have been asked of physicists and friends of physics are that they give a definite "Yes" or "No" answer to the question, "Will you help in the purchase of the building?" and that they supplement an affirmative answer with a pledge or contribution. No firm of fund-raising experts has been hired, or high power methods used. A committee of physicists has done the work.

The hope has been that practically all physicists and friends of physics will make a contribution, even though small. In some departments the response has been 100

percent, with graduate students necessarily contributing small amounts. However, many additional contributions of one to ten dollars are needed, as well as some of the larger ones. The amount required for the purchase of the property and essential repairs is \$81,000, of which \$6000 remains to be subscribed.

Those who have not yet responded are urged to send a contribution, no matter how small, to the Building Fund Committee, American Institute of Physics, 57 East 55 Street, New York 22, N. Y., or to the Committee chairman, Dr. H. L. Dodge, Norwich University, Northfield, Vermont.

NOTES AND DISCUSSION

The Carpenters' Rule: an Optical Instrument!

PAUL KIRKPATRICK

Stanford University, Stanford University, California

A CARPENTERS' rule of the folding type is a valuable aid in teaching the theory of the diffraction grating. Each segment of the rule may be taken to represent the amplitude vector contributed by a single grating element and the angles at the joints between adjacent segments may be set to correspond to any desired phase difference between contributions. The instant adjustability of the rule enables the lecturer to animate the usual static diagrams of book or blackboard, yet the figure will retain a chosen configuration. Regular polygons illustrate the diffraction patterns of perfect gratings, while distorted ones may be made to show the effects of spacing errors.

With the rule in his own hands, a moderately alert student will readily discover the number of subsidiary maxima lying between the principal maxima of the diffraction patterns of gratings of two, three, four or more elements.

An Elementary Brewster's Law Experiment

J. G. BECKERLEY, E. B. MESERVEY AND B. A. WOOTEN, JR.*
Columbia University, New York, New York

THE quantitative determination of the polarizing angle is generally left to advanced students in physics, although the phenomenon is so fundamental that it is desirable to include it in an elementary laboratory course. If first-year students are to perform the experiment, the apparatus must be simple in construction and operation. We believe the experiment described herein fulfils these requirements.

In the set-up now in use for V-12 students in General Physics at Columbia College, the equipment consists of a source, a plane reflector, an analyzer mounted on a vertical stand and a meter stick. The only measurements are the distances a and h shown in Fig. 1.

A source S is supported so that it is approximately 30 cm above the table top. Then a Polaroid P is mounted on a vertical stand and its height adjusted so that the center of the Polaroid is on a level with the center of S .

Next, a glass plate GG' is placed on the table top and the Polaroid with stand is moved to a position from which the reflected image of the source may be observed upon sighting through the Polaroid, as in Fig. 1. The axis of the Polaroid

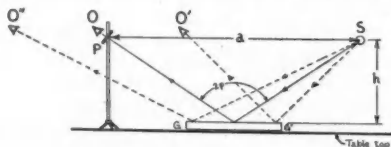


FIG. 1. Brewster's law experiment.

* Publication assisted by the Ernest Kempton Adams Fund for Physical Research of Columbia University.

is set to transmit light with vertical electric vector. By moving the Polaroid stand back and forth on the table, a position is found for which the intensity of the observed reflected image of S is a minimum.

In order to be certain that this position is truly the minimum position, the observer must move the Polaroid stand forward and backward to positions O' and O'' and observe that in each of these positions the intensity of the image is greater than the intensity at O . This "feeling-out" of the position of minimum intensity is essential to the accuracy of the experiment.

Once the position O is determined, the verification of Brewster's law follows from the measurement of the distances a and h , since $\mu = \tan p = a/h$. The validity of this equation depends on the plane of GG' being perpendicular to h . In practice this is not critical, and a visual judgment of perpendicularity is sufficiently accurate.

Similar results can be obtained by using a pan of water instead of a glass plate. The bottom of the pan should be fairly dull, to reduce the confusion caused by irrelevant reflection. Likewise, in using a glass plate a fairly dark table top is convenient.

The source S can be a tungsten lamp (frosted bulb), since the dispersion effect is too small to be accurately measured by this apparatus. If desired, a horizontal slit can be used to define S , although this does not contribute appreciably to the precision of the results.

The arrangement shown in Fig. 1 is not the only one possible. For example, S and P can be on different levels, in which case measurement of their respective levels, say h and h' , suffices to determine μ , since $\mu = \tan p = a/(h+h')$.

The procedure described has been used during the past 18 months. Experience shows that the experimental error in μ is usually less than 3 percent. Most of the error is caused by the difficulty in locating the rather broad minimum position.

While the details of this experiment represent a joint contribution of those whose names appear, credit belongs to an anonymous student in one of the laboratory sections for the idea of eliminating the clumsy sighting device which we had previously arranged.

Summary of Comments, with Discussion, on Proposed Criteria for Self-Rating of Physics Departments

WHEELER P. DAVEY

Pennsylvania State College, State College, Pennsylvania

AT the Rochester meeting of the Association¹ the writer gave by invitation a paper on criteria by which it was hoped that physics departments might, along certain lines at least, be enabled to rate themselves. In response to the Editor's request for comments,² some 60 requests for mimeographed preprints were received. This was in addition to about 30 copies which were distributed at the Rochester meeting, so that upwards of 100 physics depart-

ments have had access to the details of the proposed criteria. These departments are situated in all sections of the country and include large and favorably known state universities, small denominational liberal arts colleges, medium and large sized endowed liberal arts colleges, and heavily endowed universities. They may therefore be assumed to represent a typical cross section of American college physics departments. As a result of these hundred or so preprints, ten letters containing various amounts of adverse criticism have been received from college professors of physics and one from an industrial physicist. It is the purpose of the present paper to summarize these criticisms and to comment on some of them.

All but one of these criticisms had to do with undergraduate work.³ The following summaries and comments are therefore grouped under the same Roman numerals as appear in that portion of the published paper⁴ which deals with "Bachelor's Work."

I. The Basic College Physics Course

One liberal arts college proposes that names of textbooks be omitted and that there be substituted a statement that the course shall be "of the standard of the usual college texts." In direct contrast to this, another liberal arts college wants a complete syllabus of topics to be covered in the basic course, with indications as to the relative importance of each topic and sub-topic.

A well and favorably known privately endowed university wants provision for a purely lecture-type course with no recitation or quiz classes at all. It also wants provision for a four-semester basic course as well as for the customary two-semester course. It seems to the present writer, however, that in less steady and skilful hands both of these proposals would be subject to considerable abuse, especially in the case of an impecunious college whose administration is trying desperately to keep down the "cost per student-hour." Perhaps it is too much to expect any rating scheme to apply in all respects to all possible teaching frameworks. Probably we should be content to have a rating scheme that fits 99 percent of our physics departments.

Two liberal arts colleges want a smaller penalty for not keeping laboratory work closely in phase with the lecture and class work. They point out the supposed advantages of having the student meet the various topics in physics first in the laboratory. This appears to the present writer to be making a virtue of necessity. The writer has yet to hear of a physics department that leaves its duplicate sets of apparatus on the stock-room shelf in order to force its students to work on experiments far in advance of the lecture schedule. Instead, every laboratory manual gives a short lesson on the theory of each experiment *before* it describes the experiment itself, so that the difficulties caused by being out of phase with the lectures may be minimized as much as possible. The penalties for having laboratory work too seriously out of phase with the lectures and recitations were made rather heavy in order to help departments to bring pressure on their college administrations for sufficient funds to purchase adequate laboratory equipment.

II. Advanced Courses in Physics

One liberal arts college wants to add acoustics to the list of advanced physics courses for which "rating points" may be obtained. The writer can see no possible objection to this proposal, especially if the physics of music is included in the course.

One liberal arts college wants to raise the "points" for a three-credit course in chemical physics from 1 to 3. This proposal may be met by the fact that an adequate presentation of physical chemistry and chemical physics (see the content of books such as Getman-Daniels and McDougall) cannot be given in a three-credit, one-semester course. Another liberal arts college wants to substitute atomic physics for chemical physics. In the opinion of the present writer, a modern course in physical chemistry and chemical physics includes about as much atomic physics as the average undergraduate physics major can take to advantage. It would be most unfortunate to deprive our undergraduate physics majors of a good grounding in the rest of chemical physics—laws of gas pressure, vapor pressure, mass action, solutions, electrolytics, phase rule, colloids (more accurately known as the physics of interfaces), photochemistry, and the chemical applications of thermodynamics.

One liberal arts college wants intermediate "rating point values" for four-credit advanced courses, and complains that the requirements of Sec. II (g) are too high. One privately endowed university thinks that the proposed scheme of topics of advanced courses is too rigid. This same institution wants to schedule a course in "Slater and Frank" in the senior year, presumably at the expense of some of the fundamental work listed under (g).

III. Chemistry Courses

Most chemistry departments require quantitative analysis as a prerequisite for physical chemistry. This is objected to by a very favorably known liberal arts physics department. The present writer feels that this objection is well taken, and that physical chemistry and chemical physics can be taught quite effectively to physics majors with quantitative analysis as a parallel subject. In the writer's own institution, physical chemistry is taught by the chemistry department, and chemical physics by the physics department—each with its own prerequisites. At present, these two courses have their laboratory work in common.

One eastern liberal arts college which apparently follows the scheme of almost unlimited electives does not object to chemistry *per se* but does object to the amount of work (especially in chemistry) to be *required* of physics majors. They fear that to *require* work outside of the physics department would result in their students electing to major in other fields. But it has never seemed to the present writer that a program of study was ever any the less "intellectual" or "liberalizing" merely because it was systematic enough and thorough enough to lead to a job or to prepare a student for graduate work. A purely "a la carte" offering of courses usually encourages the average student to shop around for courses in which the highest grades can be had for the least work.

One midwestern liberal arts college wants to substitute languages for "advanced chemistry." It was never intended that the study of English and at least one other modern language should be omitted from the program of a physics major, but it was not considered necessary to bring non-science departments into a rating scheme for physics departments. It should be emphasized that the proposed rating scheme does not require a physics major to take any "advanced" courses in chemistry. In a very real sense, inorganic chemistry, quantitative analysis and organic chemistry together constitute a single five-semester course in elementary chemistry. It is of the utmost importance that, from now on, all physics majors (except those who have electrical engineering as a minor subject) get this five-semester training in elementary chemistry. The reasons for this strong statement are well known and need not be repeated here.⁵

IV. Mathematics Courses

One western liberal arts college wants provision made for so-called "unified" courses in mathematics. The present writer is not sufficiently acquainted with such courses except to say that he hopes that they work out better than the average "survey course in physics, chemistry, astronomy and what-have-you." An excellent eastern liberal arts college complains that mathematics departments tend to set too high prerequisites for their advanced courses. This tendency may be caused by an effort to insure that mathematics students have considerable "mathematical maturity." Perhaps they might be persuaded to substitute "physical and mathematical maturity."

V. Training of Physics Faculty; Teaching Loads

The reason for putting limitations on the number of advanced courses to be taught by any one man is to protect college physics teachers from the impossible task of having to become (or appearing to be) experts in the details of all branches of physics. The writer knows of one teachers college with 250 full-time students which lists in its 1944-45 catalog 20 courses in physics, chemistry, astronomy and geology. Close examination of the catalog shows that all 20 courses are to be taught by a single man. Most of these courses are listed in the catalog as three-credit courses with lectures and laboratory work. In the writer's opinion, this not only represents exploitation of the faculty member, but is a flagrant example of educational fraud which should be severely dealt with in any rating scheme. Apparently the easiest way of enforcing educational honesty in such matters is to bring such situations out in the open by requiring the name of the instructor to be printed in the college catalog immediately after the description of the advanced course which he is supposed to teach. One western liberal arts college, missing the point entirely, suggests giving a bonus of extra rating points to a physics department whose staff members take on a load of advanced courses in addition to their regular full teaching load.

A well-known Pacific Coast liberal arts college asserts that the phrase "successful teaching experience" is too indefinite. The present writer would be inclined to accept

any reasonable interpretation or range of interpretations of "successful," and would be glad for suggestions for a better phrase.

A privately endowed university suggests that "clock-hours" of teaching load ought to be weighted to take account of the extra time required for preparing lectures in advanced courses. The suggestion has merit, especially if it does not involve too many complications.

One university wonders why rating points are assigned for membership in one or more of the Founder Societies of the American Institute of Physics. It should not require much argument to show the advantages to physicists of having the practice of physics universally regarded as one of the learned professions (distinct from teaching) along with medicine, chemistry, and so forth. This can hardly be brought about by the scattered efforts of individuals acting alone. It is rapidly being brought about by the combined efforts of the five Founder Societies acting through the American Institute of Physics. All reputable physicians express their professional pride and their professional solidarity by joining the American Medical Association. Similarly, chemists express their professional pride by joining the American Chemical Society—so much so that practically all employers of chemists prefer to hire members of the Society. The least a physicist can do is to join one or more of the five Founder Societies whose efforts are giving him a profession and not just a job. The college physics teacher whose pride in his profession and in his professional society shows in his voice and in his enthusiasm in every lecture cannot help but be a better teacher. Such a man should be of more value to his department, too, because he has gained in self-respect by the knowledge that by his annual dues he is paying his share of the cost of running his professional society.

General criticisms

(1) Four members of the physics department of a large privately endowed eastern university state their opposition to any objective type of rating. They prefer "teaching and research reputation" and "quality and quantity of accomplishments of faculty and former graduates." This same view, expressed in different words, is held by the one industrial physicist who sent in criticisms. Obviously such a rating only shows what a department used to be like, not what it is like now. It makes it too difficult to rate fairly a department that has lately been greatly strengthened or greatly weakened. It offers too good a chance for the man who cannot possibly imagine that "any good thing can come out of Nazareth." The present writer believes that with all its faults and with all its obvious one-sidedness, his proposed set of criterions is more fair and more reliable than any purely subjective basis of rating. Surely the phrase "successful teaching experience" (see SEC. V, reference 4) includes some reference to "teaching reputation," and being elected a Fellow of the American Physical Society represents some little reputation in research. Even the grossly materialistic portions of the proposed criterion—apparatus, teaching loads, courses offered—have, in the long run, some bearing on the "intangible, hard-to-measure, quantities." A department with good equipment and good

teaching conditions should be able to attract good, successful, enthusiastic teachers in competition with a poorly equipped department having poor teaching conditions. Surely very few enthusiastic teachers, when confronted with such a choice, would deliberately choose the poorly equipped department. It is, as the industrial physicist points out, in the very nature of the proposed criterions that, at least within limits, the more financial support a department has, the higher rating it is able to earn. The writer finds it hard to consider adequate financial support a sin, and institutional poverty a virtue. It is the writer's fervent hope that the proposed rating criterions will enable many a poorly supported physics department to put pressure on its college administration for adequate funds.

(2) One liberal arts college suggests the addition of a rating for the "accomplishments of students." It would be almost impossible to find a college that cannot point with pride to at least one famous alumnus. And, of course, every college, no matter how good, has certain alumni about whom the less said the better. In this connection one is reminded of a remark by the late F. K. Richtmyer: "No school, no matter how poor, can do much harm to the top 10 percent of its students, and no school, no matter how good, can do much good to the bottom 10 percent. It is the in-between 80 percent for whom the quality of the school is important."

Another liberal arts college of excellent reputation proposes that ratings be established for "accomplishment" in terms of the ratio of Ph.D.'s to A.B.'s among its recent physics majors. This recommendation deserves serious study.

(3) A liberal arts college suggests that rating points be given for quantity and quality of lecture demonstration equipment and for an adequate departmental library. The only objection the present writer has to the suggestion is the difficulty of setting up reasonably fair criterions. For instance, what is the optimum relationship between lecture-room dimensions and the size of lecture-demonstration apparatus? How can the use of contrasting colors be evaluated? Should the departmental library be rated in terms of the number of books and periodicals and, if so, should the same yardstick be used for the libraries of liberal arts colleges and of technical schools? Or should a library be rated in terms of the voluntary use that is made of it by physics majors?

(4) Several liberal arts colleges seem not to have noticed the definition of a "physics major." As a result they seem to feel that SECS. II, III and IV apply to majors in other fields who take more than the basic course in physics. For instance, one college feels that the standards set by the proposed criterions are too high because so many of their "physics majors" enter the ministry instead of becoming professional physicists.

(5) The complaint is made by two or three liberal arts colleges that the proposed work to be required of physics majors is in excess of the customary liberal arts schedules of study. The present writer has prepared a suggested list of courses for liberal arts physics majors which totals 30 credits of physics, 18-22 credits of mathematics, 21-25

credits of chemistry, and 51 credits of languages and electives—a total of 120-128 credit hours. Appended to this list is an eight-semester program which shows that the suggested courses can be scheduled in such a way as to have each course preceded by its proper prerequisites. Copies of this list and program will be sent to anyone upon request.

¹ Am. J. Phys. 12, 238 (1944).

² Am. J. Phys. 12, 209 (1944).

³ This one exception merely raised the question as to why a physics department offering work leading to a Ph.D. degree ought to have glass-blowing facilities for its students.

⁴ W. P. Davey, Am. J. Phys. 12, 353 (1944).

⁵ See, for instance, W. P. Davey, Am. J. Phys. (Am. Phys. T.) 6, 11 (1938).

Additional Comments on Proposed Criterions for Self-Rating

THE editors of the *American Journal of Physics* have received a number of comments on Professor Davey's proposed criterions for self-rating of physics departments,¹ in addition to those sent directly to Professor Davey and discussed by him in the foregoing article. These additional comments are summarized herewith.

Several colleges feel that the proposed criterions seem to be best fitted to the work of a technical institute, to the disadvantage of the liberal arts college. A New England college believes that the definition of a physics major is too narrow, since a liberal arts college has many majors who do not intend to become professional physicists, and such a college should not confine itself to preprofessional training. A west coast college suggests that the number of hours set forth as the optimum is too large for most liberal arts colleges to meet.

A large and famous eastern university finds that under the proposed ratings it could not qualify in class A_B nor give work leading to a Ph.D., partly because of a difference in the way in which its undergraduate courses are organized and partly because in its undergraduate work it stresses education rather than training for a profession. On the other hand, a large western state university considers that the plan is applicable only to colleges and that it does not offer an adequate criterion for a first-class department in a university.

Several colleges find fault with specific details. A college in the Middle West wants additional credit for offering a six-hour course in basic physics, since it finds that this much time is needed to make up for poor secondary school training in mathematics and to permit aid in studying. Several liberal arts colleges feel that the course requirements, particularly in chemistry, are much too high. It would be impossible for these colleges to qualify under the proposed plan.

A southern university does not wish to be penalized because a member of the physics department is also teaching applied mathematics, becoming thereby only a part-time instructor in physics although most of his students are physics majors. This college also feels that allowance should be made for the fact that, in institutions with small classes, better, because more individualized, instruction is possible. An eastern liberal arts college believes that the

teaching load specified is too high; this is also emphatically the opinion of a large western university. An eastern women's college wants provision in the rating scheme for courses intended for prospective teachers of physics.

An eastern liberal arts college of high standing feels that the essentials of a good department are a first-class introductory course, small size of all classes after those in the first course and an interest in research on the part of the staff; it is added that the last requirement does not necessarily mean research and publication, but aliveness on the part of the teacher to what is going on in his field. This college also says that in any self-rating scheme

consideration should be given to salaries and departmental budget.

Two large and famous universities, one eastern and one western, object to the whole idea of such a plan, as encouraging rigid and formalistic mediocrity. A midwestern state university believes that such intangibles as character, ability, personality, which cannot be rated objectively, are some of the main factors in a teacher's success, and that it is dangerous to rely only on such easily measured qualities as the proposed plan is based upon.—JOSEPH D. ELDER.

¹ W. P. Davey, *Am. J. Phys.* 12, 353 (1944).

Proceedings of the American Association of Physics Teachers

The New York Meeting, January 18-20, 1945

THE fourteenth annual meeting of the American Association of Physics Teachers was held at Columbia University, New York City, on January 18 to 20, 1945. The presiding officers were L. W. Taylor, President of the Association, and Louise S. McDowell, Vice President. Professor McDowell was also chairman of the program committee for the meeting.

A joint dinner with the American Physical Society was held at the Men's Faculty Club of Columbia University on Friday evening, January 19.

Invited Papers

Four sessions were devoted to the following invited papers:

Symposium on Design of Instructional Laboratories

Functions of the housing committee. W. P. DAVEY, *Pennsylvania State College.*

Floor plans. C. J. OVERBECK, *Northwestern University.*

The lecture room and its equipment. J. W. BUCHTA, *University of Minnesota.*

Laboratory electric services. C. E. HOWE, *Oberlin College.*

Symposium on the Place of Physics in a Liberal Arts Education

Introductory remarks. H. L. DODGE, *Norwich University.*

The physical sciences in general education. AARON SAYVETZ, *University of Chicago.*

A cultural course in college physics for nontechnical students. W. S. WEBB, *University of Kentucky.*

Physics in the liberal science program at Purdue University. KARL LARK-HOROVITZ, *Purdue University.*

Some values of problems and laboratory for the non-scientific. S. R. WILLIAMS, *Amherst College.*

Mathematics for the student of elementary physics. R. B. LINDSAY, *Brown University.*

Physics and the history of ideas. WAYNE B. DENNY, *Oberlin College.*

Physics as a liberating discipline. HENRY MARGENAU, *Yale University.*

Selling physics to the faculty. WALTER G. CADY, *Wesleyan University.*

Joint Session with the American Physical Society

Twenty-five years of mass spectroscopy. A. J. DEMPSTER, *University of Chicago; retiring president of the American Physical Society.*

Radiofrequency spectroscopy. I. RABI, *Columbia University; third Richtmyer memorial lecturer of the American Association of Physics Teachers.*

Presentation of the Oersted Medal to Homer Levi Dodge. A. A. KNOWLTON, *Reed College*, and L. W. TAYLOR, *Oberlin College.*

Demonstration Lecture

Demonstrations for color and color photography. H. C. COLTON, *University of Rochester.*

Symposium on Technical Writing and Editing

Source literature and elementary textbooks. DUANE ROLLER, *Wabash College.*

Purpose as a factor affecting the form and content of technical reports. DEANE R. WHITE, *E. I. du Pont de Nemours & Company.*

Giving power to words. PHILIP W. SWAIN, *Editor of Power.*

Multa verba dele. JOHN MILLS, *Bell Telephone Laboratories.*

Contributed Papers, with Abstracts

Two sessions were devoted to the following contributed papers:

1. Can the Association recognize and encourage young teachers? ROBERT S. SHAW, *College of the City of New*

York.—The Oersted medal, honorary degrees and other kudos are given, from their very nature, at the culmination of an extended career of teaching. They do not serve to attract young persons into the teaching profession, nor to encourage anyone to shift the emphasis of his work in the direction of teaching. The Association was, in the nature of things, founded by men past middle age. However, the author believes that a healthy condition for the organization requires that new members join early in their careers. In his direct experience this has not happened; instead, membership is put off until middle age. The difficulty appears to be partly the result of the fact that no one can be recognized as a promising young teacher, as, for example, one may be known as a promising research man by membership in Sigma Xi. The question of similar recognition for young teachers is raised, but not answered, here. The matter may become of importance if industry competes with teaching for the new crop of physicists in the postwar years.

2. A logical approach to the subject of units and dimensions. DONALD ROSS, *Harvard University.*—In order to understand better the many different systems of units in use in physics and engineering, it seems necessary to broaden the concepts of such a system and of a physical law. We should include a constant of proportionality in the general form of all physical laws that relate different physical quantities. We should then define a system of units not only by the choice of fundamental units, but also by the choice of values of the proportionality constants in important physical laws. Thus if, in a given system of units, the units of all the quantities occurring in a given experimental law have been previously defined, then the law defines the constant of proportionality as to both magnitude and units. On the other hand, if in the given system of units the unit of one of the quantities appearing in the law is as yet undefined, then this unit can be defined by an arbitrary choice of the proportionality constant, often as unity. Different choices of the constant of proportionality give essentially different systems of units.

3. Rationalization of equations in electromagnetism. H. JEHL, *Harvard University.*—Translation from the rationalized form of the magnetic circuital law to the non-rationalized (in mks units) is always performed by changing the unit of H from 1 amp turn/m to 1 millioersted, using the conversion formula, 1 amp turn/m = 4π millioersted, keeping the physical quantity H unchanged; for example, $H = 37$ amp turn/m = $4\pi \times 37$ millioersted. In order to convert the rationalized formula (between numerical values) to nonrationalized, we simply substitute 4π millioersted and get

$$Ni = \oint \mathbf{H} \cdot d\mathbf{s} = \oint \mathbf{H} \cdot d\mathbf{s},$$

(amp) (amp turn/m) (m) (4π millioersted) (m)

which, after multiplying left- and right-hand members by 4π , represents the nonrationalized form. Similarly we propose to go from the rationalized form of Gauss' law to the nonrationalized by substituting a new unit of the

unchanged physical quantity D , 1 coul/m² = 4π lorentz/m², that is,

$$q = \oint \oint \mathbf{D} \cdot d\mathbf{S} = \oint \oint \mathbf{D} \cdot d\mathbf{S}.$$

(coul) (coul/m²) (m²) (4π lorentz/m²) (m²)

4. Introduction of magnetic and electric physical quantities. J. G. WINANS, *University of Wisconsin.*—Teaching physics understandably requires that only one definition be given for each physical quantity. It is further desirable that definitions be as closely related to experiment as possible. Electric charge may be introduced as an undefined quantity which makes small objects exert forces according to the law $Fd^2 = K$ in a vacuum; K may then be assumed to equal Q_1Q_2 and unit charge esu established. A consistent system of single definitions then follows with the defining equations:

field strength = F/Q , with charge small;
 potential difference $V = \text{work}/Q$, capacitance $C = Q/V$;
 resistance $R = V/I$; inductance $L = Vdt/dI$.

Magnetic pole strength can be considered in the same manner as charge, with K now put equal to m_1m_2 . This gives magnetic field strength $H = F/m$, and magnetic moment $M = ml$. Electromagnetic units may be introduced through the experiment which shows that $H/I = 3 \times 10^{10}$, where H is the field strength at the center of an arc of wire 1 cm long and 1 cm radius in which the current is I . Then, introducing a new system for which $H/I = 1$, we have the conversion factors established between esu and emu. It is important that the definitions of quantities be the same in both systems, with only the unit being different. This has not been customary practice in physics textbooks.

5. To the master's degree in four years. ALAN HAZELTINE, *Stevens Institute of Technology.*—The complete article will appear in the next issue.

6. Wave velocities in elementary physics. S. MILLMAN, *Queens College*, and M. W. ZEMANSKY, *College of the City of New York.*—The complete article will appear in a later issue.

7. A centripetal force experiment. W. C. KELLY AND E. S. MESSER, *University of Pittsburgh.*—A centripetal force experiment, yielding accurate results and using standard apparatus, has been devised for laboratories requiring many set-ups of the same experiment. A heavy metal ball of weight W swings as a pendulum on a circle of known radius r , attaining a velocity v at the lowest point of its path. Here the centripetal force F_c may be computed since

$$F_c = (W/g)(v^2/r) = (W/g)(2gh)/r,$$

where h is the vertical distance through which the ball falls. The ball is attached by a wire of convenient length to a hook beneath the left-hand pan of a standard trip scale mounted rigidly at the top of a long rod. Beneath the hook, the wire runs freely through a small hole in a metal plate mounted on the rod. The radius r is measured from

the plate to the center of gravity of the ball and may be varied. The student places a load on the right-hand pan of the scale and, releasing the ball from the same vertical position in repeated trials varies the load until the pointer of the scale deflects slightly as the ball swings through its central position. This load minus the weight of the ball gives the experimental value of the centripetal force.

8. Imaging of underwater objects. LAWRENCE E. KINSLER, *U. S. Naval Academy*.—Despite recent articles on the correct location of the image of an object immersed in water, incorrect theories and explanations are still too prevalent. This is particularly unfortunate when they are applied by Naval personnel in estimating the location of underwater objects. Some common textbook errors, as well as an acceptable elementary presentation, were cited. More advanced treatment shows that a pencil of rays originating from a point object in water is astigmatic upon emerging from the water. The vertical astigmatic image is directly above the object, and the horizontal astigmatic image is above and forward of the object. Measurements on displacements of images produced by a flat plate agree with theoretical predictions. Location of an image by monocular vision will thus depend on which astigmatic image is brought into focus. In normal binocular vision the apparent position of the image is that of the vertical astigmatic image. If the head is rotated so that the eyes are in a vertical plane, binocular vision locates the image at the nearer or horizontal astigmatic image. Curves showing the location of these two images of an object in water as a function of viewing angle were presented.

9. Demonstrating linear thermal expansion using the catenary. RICHARD C. HITCHCOCK, *U. S. Naval Academy*.—A horizontal wire of uniform cross section, supported by two posts, to which an electric potential difference is applied, will sag, largely owing to thermal expansion, into a catenary. Paper riders mark the position of the cold wire and burn off when the wire becomes red hot. If the span between posts is 100 cm and an initial sag of 2.0 cm is allowed in order not to stretch the wire too much at the start, an increase of wire length by 0.9 cm will give a total sag of 6.3 cm, readily visible at a distance. The elongation may be predicted by an extension of Gruneisen's law; metals expand a maximum of 2.5 percent from absolute zero to their melting points. This explains why metals with low melting points have large expansion coefficients. A minimum of equipment is needed to show this expansion to a large group; no levers, axles or pointers are used which might conceal operations.

10. Student salvage. JOHN A. TIEDEMAN, *U. S. Naval Academy*.—Midshipmen who fail and must be dropped are a serious loss in war time. Reasons for failure were analyzed and the conclusion reached that failures in physics were largely caused by lack of grasp of fundamentals. Re-teaching (amounting to about 25 percent of the time spent on the original course) was introduced for failures. On re-examinations (roughly comparable with examinations on which the students failed) remarkable improvement was

shown. Furthermore, students who failed had a previous history of consistent examination failures (had passed only by averaging class work) in other subjects. Inspection of records of later examinations in other subjects, as well as in physics, shows that students re-taught do not fail again.

11. Improvement of physics teaching in engineering schools. THOMAS F. BALL, *U. S. Naval Academy*.—Wartime conditions have made it imperative that young men who expect to enter the Naval Reserve as commissioned officers and who are qualified, be given advanced training in the fundamentals of electrical engineering, including direct and alternating currents, radio, electronics and radar. Such a course was devised for the training of Reserve Midshipmen at the U. S. Naval Academy. The groups, coming from engineering schools all over the country, totaling 3315 individuals and averaging 332 per group, have furnished the Naval Academy with sufficient data from which to draw definite conclusions concerning the teaching of physics in the engineering schools of the country. It is hoped that the revelation of such weaknesses, as set forth in this report, may be considered as being constructive, rather than destructive, criticism.

Attendance

The following 171 members registered at the meeting:

Mildred Allen, Mount Holyoke College; S. Anderson, University of Illinois; Gladys Anslow, Smith College; E. G. F. Arnott, Westinghouse Electric & Manufacturing Co.; B. W. Bartlett, U. S. Military Academy; H. A. Barton, American Institute of Physics; Viola Barton, Goucher College; P. F. Bartunek, National Bureau of Standards; Hilda Bass, University of Rochester; J. W. Beams, University of Virginia; P. Bender, Goshen College; C. E. Bennett, University of Maine; D. K. Berkey, Colgate University; W. H. Billhartz, Franklin College; H. Louisa Billings, Smith College; O. H. Blackwood, University of Pittsburgh; B. F. Boardman, Fresno State College; R. B. Bowersox, Harvard University; R. A. Boyer, Muhlenberg College; H. Branson, Howard University; G. P. Brewington, University of Detroit; F. L. Brown, University of Virginia; T. B. Brown, George Washington University; J. W. Buchta, University of Minnesota; G. H. Burnham, American Institute of Physics; W. G. Cady, Wesleyan University; G. H. Cameron, Hamilton College; E. C. Campbell, Princeton University; W. L. Cheney, George Washington University; S. M. Christian, Agnes Scott College; E. A. Cook, NDRC; F. P. Cowan, Harvard University; S. W. Cram, Sylvania Electric Products; K. K. Darrow, Bell Telephone Laboratories; W. P. Davey, Pennsylvania State College; Pauline Davidson, Queens College; A. J. Dempster, University of Chicago; W. B. Denny, Oberlin College; F. Dietz, Wesleyan University; E. H. Dixon, University of Georgia; H. L. Dodge, Norwich University; Elsie Dollmann, New York University; Clare Driscoll, Brookline, Mass.; L. A. DuBridge, Massachusetts Institute of Technology; Barbara Dwight, Wesleyan University; R. J. Dwyer, Trinity College; F. K. Elder, U. S. Naval Academy; R. Ellickson, Polytechnic Institute of Brooklyn; H. W. Farwell, Columbia University; C. R. Fountain, Camp Evans Signal Laboratory; W. R. Fredrickson, Syracuse University; R. C. Gibbs, Cornell University; N. E. Gilbert, Dartmouth College; Helen Gilroy, Wilson College; N. S. Gingrich, University of Missouri; G. E. Grantham, Cornell University; A. N. Guthrie, Brooklyn College; M. C. Harrington, Drew University; N. H. Hartwell, Durham, N. H.; A. Hazeltine, Maplewood, N. J.; Monica Healea, Vassar College; L. B. Heilprin, National Bureau of Standards; C. L. Henshaw, Colgate University; R. C. Hitchcock, U. S. Naval Academy; J. R. Hobbie, Skidmore College; C. E. Howe, Oberlin College; J. H. Howey, Georgia School of Technology; L. G. Hoxton, University of Virginia; H. H. Hubbell, Jr., Princeton University; G. F. Hull, Jr., Dartmouth College;

J. M. Jauch, Princeton University; G. E. Jones, Atlantic Union College; R. N. Jones, Philadelphia College of Pharmacy and Science; G. E. C. Kauffman, University of Delaware; W. Keck, Muhlenberg College; W. H. Keeble, Randolph-Macon College; H. C. Kelly, Massachusetts Institute of Technology; W. C. Kelly, University of Pittsburgh; E. C. Kemble, Harvard University; R. F. Kingsbury, University of Maine; L. E. Kinsler, U. S. Naval Academy; A. A. Klebba, Woods Hole Oceanographic Institute; P. E. Klopsteg, Northwestern Technical Institute; H. C. Knauss, Swarthmore College; A. A. Knowlton, Reed College; G. M. Koehl, George Washington University; A. Kolin, Columbia University; H. P. Krauss, Harvard Underwater Sound Laboratory; J. Kurshan, RCA Laboratories; R. T. Lagemann, Emory University; Elizabeth Laird, London, Ont.; K. Lark-Horovitz, Purdue University; Herta Leng, Rensselaer Polytechnic Institute; H. W. LeSourd, Milton Academy; R. B. Lindsay, Brown University; F. W. Loomis, Massachusetts Institute of Technology; Lilly Lorentz, Smith College; F. E. Lowance, Harvard University; W. N. Lowry, Bucknell University; Louise McDowell, Wellesley College; D. R. McMillan, Jr., Emory University; J. H. McMillen, Princeton University; K. V. Manning, Pennsylvania State College; A. E. Martin, A. D. Cardwell Manufacturing Corp.; M. Martin, University of Wisconsin; Helen Messenger, Hunter College; W. C. Michels, Navy Department; W. H. Michener, Carnegie Institute of Technology; J. Mills, Bell Telephone Laboratories; Madeline Mitchell, American Institute of Physics; R. Morgan, University of Maryland; L. B. Morse, College of the City of New York; R. D. Myers, University of Maryland; M. E. Nelson, Navy Department; W. Noll, Berea College; P. A. Northrop, Vassar College; T. H. Osgood, Michigan State College; H. N. Otis, Hunter College; C. J. Overbeck, Northwestern University; S. Pasternack, University of Pennsylvania; R. F. Paton, University of Illinois; G. B. Pegram, Columbia University; L. W. Phillips, Armstrong Cork Co.; Melba Phillips, Brooklyn College; W. B. Pietenpol, University of Colorado; M. L. Pool, Ohio State University; J. G. Potter, Bell Telephone Laboratories; W. P. Reid, New York University; E. M. Rogers, Princeton University; D. Roller, Wabash College; A. Romer, Vassar College; G. K. Schoepfle, Oberlin College; G. Schwarz, University of Illinois; N. S. Shankland, Case School of Applied Science; R. S. Shaw, College of the City of New York; Sister Grace Marie, College of Chestnut Hill; L. E. Smith, Denison University; L. P. Smith, Cornell University; R. D. Spangler, E. I. du Pont de Nemours & Co.; A. H. Spees, Wesleyan University; M. N. States, Central Scientific Co.; J. C. Stearns, University of Chicago; G. W. Stewart, University of Iowa; Hildegard Stucklen, Sweet Briar College; R. M. Sutton, Haverford College; L. W. Taylor, Oberlin College; J. A. Tiedeman, U. S. Naval Academy; M. H. Trytten, National Research Council; F. G. Tucker, Oberlin College; P. L. Vissat, Louisville, Ky.; R. F. Warren, Massachusetts Institute of Technology; B. B. Watson, University of Pennsylvania; W. S. Webb, University of Kentucky; R. L. Weber, Pennsylvania State College; Dorothy Weeks, Washington, D. C.; M. R. Wehr, Swarthmore College; F. Weinrich, Brooklyn College; M. W. White, War Department; A. O. Williams, Jr., Brown University; S. R. Williams, Amherst College; C. Williamson, Carnegie Institute of Technology; J. G. Winans, University of Wisconsin; B. F. Wissler, Middlebury College; E. E. Witmer, University of Pennsylvania; K. S. Woodcock, Bates College; A. G. Worthing, University of Pittsburgh; W. R. Wright, Swarthmore College; V. J. Young, Sperry Gyroscope Company; K. Yudowitch, Columbia University; J. Zeleny, Yale University; M. W. Zemansky, College of the City of New York.

Report of the Secretary

The executive committee of the American Association of Physics Teachers held two meetings in New York City on January 18 and 19, 1945. President Taylor presided. Some seven hours were needed to dispose of the considerable amount of business that had accumulated during the year.

Members of the executive committee who attended were: *Gladys Anslow, *L. I. Bockstahler, P. E. Klopsteg, A. A. Knowlton, *K. Lark-Horovitz, *Louise McDowell, *W. H. Michener, W. Noll, *C. J. Overbeck, R. F. Paton, W. B. Pietenpol, *D. Roller, *L. W. Taylor, J. A. Tiede-

man, *M. R. Wehr and *A. G. Worthing. Others who attended by invitation were: H. A. Barton, T. D. Cope, W. P. Davey, H. L. Dodge, *R. C. Gibbs, J. H. Howey, H. K. Hughes, W. N. Lowry, *T. H. Osgood, J. G. Potter, F. O. Schmitt, M. N. States, R. M. Sutton and M. W. White. Asterisks indicate the names of those who attended the second meeting as well as the first.

Business with American Institute of Physics.—It was reported that, as the result of a mailed ballot taken in September 1944, the executive committee has adopted the following resolutions:

I. That, subject to similar and concurrent action by the governing board of each of the other Founder Societies of the American Institute of Physics, the American Association of Physics Teachers agrees to contribute annually to the support of the Institute of Physics in proportion to this society's income received in the preceding year as dues of individual members, of all classes, of the society, provided: (a) that each society shall contribute the same fraction of its similar income; (b) that the said fraction shall not exceed 15 percent; and (c) that the fraction to be paid each year be set by the Governing Board of the Institute of Physics and that notice of the fraction for each following year be sent to the secretaries of the societies prior to September 1.

II. That this agreement as to contributions to the American Institute of Physics shall be effective from the beginning of the calendar year following the receipt by the secretary of this society of official notice of (a) similar and concurrent action by each of the other Founder Societies, and (b) action by the Institute of Physics to release each of the Founder Societies from the 15-percent service charge provided in publication contracts with the Institute.

III. That this agreement as to contributions to the American Institute of Physics shall not be terminated by any of the member societies without at least a full year's notice to each of the other member societies and to the Governing Board of the Institute of Physics.

The Institute of Physics has been notified of this action, and the plan is now in force.

H. A. Barton presented the problem of how to handle requests for gifts of subscriptions and back numbers of journals received from liberated and enemy countries; since any policy concerning such requests should be uniform for all the physics journals, it was agreed that the Institute should settle the matter. Doctor Barton gave a brief and encouraging report on progress made in financing the new Institute headquarters. He asked for consideration of an effective program for discovering and developing scientific aptitude in youth so that research may progress in the postwar years. He also indicated that there may soon be a demand for physicists for temporary service as teachers in foreign service and urged that persons available communicate with him.

It was voted to cooperate in carrying forward the ideas expressed in the following recommendations of the Governing Board of the Institute: (i) that the Institute be authorized to arrange an annual combined meeting of the member and associated societies for all persons interested in physics and its contribution to other fields; (ii) that in any case of a proposal for the formation of a special division of a society, the governing body of that society consider whether such action may be in conflict with the activities of other member societies and whether the proposed division will serve the best interests of organized physics as a whole; and, if there is possibility of a conflict, that

representatives of the societies involved discuss the matter fully before a decision is made.

P. E. Klopsteg was nominated to succeed himself as a representative of the Association on the Governing Board of the Institute for another three-year term.

Reports of officers.—Each officer made a brief report. The secretary reported that expenses for his office in 1944 were considerably less than the \$750 set aside for this purpose, but recommended that the same amount be allotted for 1945 because of possible additional expenses.

The journal.—D. Roller was reappointed editor of the AMERICAN JOURNAL OF PHYSICS for a term of three years. Upon his recommendation, R. L. Edwards and R. B. Lindsay were appointed associate editors, with terms of three years. Upon recommendation of the committee to formulate a policy for financing the journal, the sum of \$1200 was set aside for editorial and traveling expenses, with the expenditures to be made at the discretion of the editor. After lengthy discussion of problems which might arise if the Institute of Physics instituted a general journal of physics, as has been proposed, it was voted that each member of the executive committee present his views and possible solutions in writing, and that a committee then be appointed by the President to study these and formulate a report to the Institute.

Committee on membership.—R. C. Gibbs reported that there were 131 new members in 1944 and already 55 for 1945; the total membership is 1140, as of January 18, 1945.

Committee on cooperative action.—Upon the recommendation of this committee, as presented by K. Lark-Horovitz, it was voted: (i) that the name of the committee be changed to "Committee for the teaching of physics and physical sciences in the secondary schools"; (ii) that the membership of the committee be enlarged so as to include not only members of the Association but also representatives from secondary schools and from industry, and that consultants in various fields be appointed to cooperate with the committee in an advisory capacity; (iii) that definite proposals for teacher-training plans and comprehensive examinations for teachers be submitted to subcommittees of regional chapters so as to stimulate activity of the chapters in this matter and to promote upgrading of both the formal and in-service training of teachers; (iv) that the committee cooperate with similar groups in other fields of science. Approval was given to the complete, written report submitted by this committee. The sum of \$250 was authorized to help defray committee expenses.

Committee on letter symbols and abbreviations.—H. K. Hughes reported that the fourth report of this committee is about ready for publication; also that recent editions of the *Handbook of chemistry and physics* contain the list of symbols proposed by the committee in its third report.

Engineering education.—H. L. Dodge reported that the SPEE has created a division of physics. Upon his recommendation, it was voted to establish the policy of holding an annual summer meeting in conjunction with the SPEE. It was agreed to be generally unwise for the Association to hold two summer meetings, especially if they are close together in time and location.

Other committees.—Written reports or brief oral reports

were accepted from the chairmen of committees on awards (A. A. Knowlton), electric units (E. C. Kemble), Richtmyer Memorial Lecture (F. Palmer) and terminology (D. Roller).

Regional chapters.—All nine regional chapters of the Association filed their annual reports for 1944. The petition of the Southern California Association of Physics Teachers for recognition as the tenth regional chapter was granted by a unanimous vote.

Regional meetings.—Two meetings were held in 1944: at Rochester, concurrently with the American Physical Society, June 23–24; and at Cincinnati, jointly with the SPEE, June 22–24. [See Am. J. Phys. 12, 237 (1944)].

American Council on Education.—The Association representatives—R. M. Sutton, K. Lark-Horovitz and R. C. Gibbs—were instructed to present a written report on the 1944 meeting to the Governing Board of the Institute of Physics. The senior representative, R. M. Sutton, will be replaced, following custom, by a new representative to be appointed by the president. It was voted to appropriate \$100 to help meet the traveling expenses of these representatives.

Physics in premedical and medical training.—F. O. Schmitt, department of biology and biological engineering, Massachusetts Institute of Technology, described a report submitted to the Scientific Advisory Committee of the Baruch Committee on Physical Medicine. He suggested the needs for: (i) an elementary physics textbook oriented toward premedical and biological physics; (ii) a course in applied physics in these fields at an advanced level; (iii) the assembly of suitable experiments for the premedical course. It was voted that the president appoint a committee to collect suitable material for the manual with the help of men from medical science.

Advisory program committee.—At the suggestion of Louise McDowell, a standing committee was created for the purpose of developing a continuity of program topics for future meetings of the Association. The committee will consist of the Vice President and two members of the executive committee.

Employment of physics graduates at the bachelor's level.—A committee to study this problem and make a report was appointed as follows: M. N. States, *chairman*, G. Burnham, R. C. Gibbs, M. J. Kelley and M. H. Trytten.

National Science Teachers Association.—It was decided to affiliate with this organization by group membership, the yearly dues for which are \$2. W. H. Michener and the members of our committee on the teaching of physics and physical sciences in the secondary schools were appointed consultants to the organization, and were requested to report to our executive committee at its next annual meeting.

Future meetings.—The committee is considering possible ways of furthering the activities of the Association in the interest of the war effort, especially in view of the present Government restrictions on conventions. The secretary was instructed to request permission for the next annual meeting.

Miscellaneous business.—(a) The president was instructed to appoint our representatives for 1945 to the

AAAS. (b) After discussion of the system of balloting now in use by the Association, it was decided that the present system is adequate; it was voted that in the future the president be authorized to appoint tellers and have the ballots counted *in advance* of the annual meeting.

(c) Certain criticisms of the physics tests sponsored by the Association were discussed and were referred for action to the committee on tests and the Cooperative Test Service.

(d) It was decided to institute a budget committee consisting of the president, treasurer and two members of the executive committee appointed by the president; it is to begin work in 1946.

Annual business meeting.—In order to provide adequate time for the presentation of contributed and invited papers, the fourteenth annual business meeting, which was scheduled for 10:00 A.M., January 20, had to be limited to the report of the tellers for the annual election. After hearing the report, President Taylor ruled that the election had resulted as follows:

President: R. C. Gibbs.

Vice President: Marsh W. White.

Secretary: C. J. Overbeck.

Treasurer: Paul E. Klopsteg.

Members of Executive Committee: T. H. Osgood; A. E. Ruark.

The tellers, C. E. Howe and J. H. Howey, based their report on the count of 466 sealed ballots received by the secretary before Wednesday noon, January 17.

C. J. OVERBECK, *Secretary*

Annual Report of the Treasurer

Balance brought forward from Dec. 15, 1943..... \$2289.15

CASH RECEIVED

Dues received for 1944 ¹	\$5082.50
Dues received for 1943.....	15.00
Dues received for 1945.....	245.00
Dues received for 1946.....	10.00
Royalties, <i>Demonstration experiments</i> <i>in physics</i>	190.68

Constituent Membership in ACE, paid by American Institute of Physics.....	100.00
Donations.....	5.55
<i>Total deposited, 12/15/43 to 12/15/44....</i>	<i>5648.73</i>

Total cash available..... \$7937.88

DISBURSEMENTS

Postage and supplies.....	\$ 175.00
Printing.....	553.02
Stenographer, editor's office.....	569.40
Editor's traveling expenses.....	124.54
Secretary's office expense.....	326.29
Constituent membership in ACE... ..	100.00
Payments to American Institute of Physics.....	1508.20
Miscellaneous traveling expenses... ..	563.50
Discount on checks.....	2.13
Three medallions and cases.....	44.04
Miscellaneous secretarial service... ..	25.00
Film strips for regional meetings... ..	5.50
Miscellaneous.....	14.05
<i>Total disbursed.....</i>	<i>4010.67</i>

Balance on hand Dec. 15, 1944²..... \$3927.21

PAUL E. KLOPSTEG, *Treasurer*

I have audited the books of account and records of Dr. Paul E. Klopsteg, Treasurer of the American Association of Physics Teachers, for the year ended December 15, 1944, and hereby certify that the foregoing statement of receipts and disbursements correctly reflects the information contained in the books of account. Receipts during the year were satisfactorily reconciled with deposits as shown on the bank statements, and all disbursements have been satisfactorily supported by vouchers or other documentary evidence.

WILLIAM J. LUBY
Certified Public Accountant

Chicago, Illinois,
December 21, 1944.

¹ On Dec. 15, 1944, the number of members in good standing, including emeritus members, was 1085.

² Approximately \$1000 is still due the American Institute of Physics for the publication of the journal for 1944. In addition to the balance indicated, the Association has \$5000 invested in short-term Government bonds [see Am. J. Phys. 12, 54 (1943)].

District of Columbia and Environs Chapter

MORE than 100 persons, from some 40 institutions, attended the meeting of the District of Columbia and Environs Chapter of the American Association of Physics Teachers on November 11, 1944 at the United States Naval Academy, Annapolis, Maryland. Captain F. K. Elder, chairman of the Physics Committee of the Academy, presided. Members and guests were welcomed by Rear Admiral J. R. Beardall, Superintendent of the Academy, and Captain W. Nyquist, Head of the Electrical Engineering Department, of which the Physics Committee is a part.

The following program of contributed papers was presented in morning and afternoon sessions.

Aid to stereoscopic aerial reconnaissance. J. D. RIGGIN, *U. S. Naval Academy.*

Limit of error. F. E. FOX, *Catholic University of America.*

Demonstration of modulation theory. T. B. BROWN, *George Washington University.*

Noise and its limitation to measuring devices and amplifiers. K. F. HERZFELD, *Catholic University of America.*

Notes on the concept of mass. V. P. BARTON, *Goucher College.*

Changes advisable in the courses in electricity and magnetism. M. KATHERINE FREHAFFER, *Goucher College.*

Manpower trends. M. H. TRYTTEN, *Office of Scientific Personnel.*

Demonstrating boiling water at reduced pressure. R. C. HITCHCOCK, *U. S. Naval Academy.*

Energy in liquid films and elastic springs; some misapprehensions. L. G. HOXTON, *University of Virginia.*

Penetration ballistics. G. R. IRWIN, *Naval Research Laboratory.*

Demonstration of the schlieren effect. C. A. BECK, *Catholic University of America.*

Proposed physics building at the University of Delaware. J. F. DAUGHERTY, *University of Delaware.*

Doppler effect; a lecture demonstration. A. S. JENSEN, *U. S. Naval Academy.*

Luncheon was served at the North Severn Officers' Mess, to which the group was transported by motor launch. After the luncheon R. M. Sutton, of Haverford College, discussed demonstration experiments and presented several examples. A short business meeting followed the afternoon session, after which members were taken on a guided tour of the Naval Academy.

E. R. PINKSTON

NECROLOGY

Daniel Stanley Elliott, 1885-1944

DANIEL STANLEY ELLIOTT, head of the Department of Physics at Tulane University, was born in Baltimore on August 3, 1885. He received his training at Johns Hopkins University. During the period 1914-1920 he was at the Georgia School of Technology, where he took an active part in the training of men in radio and aeronautics for the Army. His death on December 1, 1944 was a very real loss to his colleagues and his profession in the Southeast.

DOCTOR ELLIOTT's chief interest lay in the development of young men in the field of physics, and in opening pathways for these men after their graduation. In his teaching of elementary students he placed great emphasis upon two points: a student has an inalienable right to make a reasonable number of mistakes without interference; and intelligent planning by the student should be a fundamental requirement. Graduate students were trained by assignment of reasonably difficult tasks of all kinds with a minimum of supervision, but with insistence upon a superior rather than an adequate performance. Self-sufficiency on their part was encouraged through supplementary training by specialists in a variety of laboratory technics. Visual education through films and slides was given strong emphasis on all levels.

Despite time lavished on the training of students and on his other university responsibilities, DOCTOR ELLIOTT found time to take a vigorous part in the development of the profession in his region. He was a sturdy and able

fighter for the things in which he believed, and he liked to say that he practiced the "wave-front" method of advance rather than the "ray" method. As a result, he was prominent in a number of organizations and activities. For a number of years he was active in the Society for the Promotion of Engineering Education; he was a leader in the renaissance of the New Orleans Academy of Sciences; and he was one of those instrumental in the formation of the Southeastern Section of the American Physical Society. Just prior to the outbreak of war, he was giving much thought to the development of an active chapter of the American Association of Physics Teachers for Louisiana.

When the Naval V-12 training program was inaugurated at Tulane, it fell to him to cope with the sudden, fourfold expansion of his department to meet the needs of the program. This great task was accomplished, but not without a drain on his energies beyond his recuperative powers. After the peak of the load had been successfully passed, he was ordered to his home by his physician for a rest.

During the weeks of enforced inactivity before his unexpected death, he still found it possible to plan for the future and was at work on a study of trends in physics, and on plans for the education of returning veterans. His death thus came while he was still in a sense in harness—as he would have wished it. His memorial will be a growing thing through those who have worked under and with him, and who have profited by his fearless honesty, his sincere and practical interest in others, and his wise and clear-sighted counsel.

E. S. BARR



IN the course of experience of many generations of students, I have known far more to fail from lack of grit and perseverance than from the want of what is commonly called cleverness.—J. J. THOMSON.